

# INTERSTATE MEDICAL JOURNAL.

**Vol. XVI.**

**JULY, 1909.**

**No. 7.**

## EDITORIAL.

### A NEW RUBRIC IN THE DETERMINATION OF SEX.

If there is one subject in medicine that shows an efflorescence indicative of a perennial freshness, it is the all-absorbing one of the reasons why necessitous families are burdened with a superabundance of female children, while those who live in affluence and could well afford to have more daughters than sons, have the power to propitiate an unkind fate in some mysterious way, and alternate the female product with the male. That the matter is not altogether in the hands of Clotho, has been explained to us before by various visionaries who, by means of a special diet, were ambitious to wrest an iota of the honor of arranging the significant matter of sex from the clutches of the Greek goddess; but though we were impressed for a time with the deep knowledge of the investigators, and hearkened not unwillingly to the lure of their theories, families refused to abide by the decisions, and the petty annoyance of too many daughters went merrily on. But human ingenuity in this matter is not easily discouraged, for every now and then a new and extraordinary theory is advanced; and while it may not prove more effective than the inspired dietary vision of Professor Schenk, it has enough of the element of the novel, to add considerably to the gaiety of physicians. France and England are linked together this time in the vision of the betterment of social conditions that would happen, should families be characterized by a preponderance of sons over daughters; but though the innovative ideas were published almost simultaneously, the English author's composition\* is so deeply and thoroughly informed with what might be called the very ecstasy of medical science, that the

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\*The Causation of Sex: A New Theory of Sex based on Clinical Materials, together with Chapters on the Forecasting of the Sex of the Unborn Child, and on the Determination or Production of Sex at Will. By E. Rumley Dawson, L. R. C. P. Lond., M. R. C. S., Eng. London: H. K. Lewis. 1909.

Frenchman's theory\* is sure to be neglected as too mild to merit critical interpretation.

It would really be a difficult matter to give, in toto, all the masterly theories which at once pass into facts by the annealing process of a quick cerebration as instanced in the ratiocination of Mr. E. Rumley Dawson; but while this would be beyond our patience, and much beyond the reader's mental endurance, it behooves us to expatiate on the most vital part of the book—the one and only fact which determines the sex of the child. Hitherto, while moral philosophers were not agreed that the male parent was entirely responsible for the sex of his offspring, they did not entirely dismiss him from their reasoning on the matter; but Mr. Dawson is of a different opinion and shows, with rare lucidity, that a spermatozoon is a negligible quantity when it comes to the consideration of the sex of the child and the transmission of hereditary qualities. What mysterious force, then, is responsible in the causation of sex, or by what mental effort on the part of the female, since she alone can predestine the matter, are results achieved that will make her a popular entity, when male children are greatly desired to feed the insatiable maw of a combination of militarism and commercialism? And by what means, at our disposal, may we throw a sop to those restive sociologists, who are continually crying out against modern civilization because, forsooth, there has always been the dread with us, that a considerable number of non-earning daughters to each family must be a drag on all future prosperity? Shunning devious ways, as unworthy the clarity of the profound thinker, our author allies himself at once with the positive school of medical investigators, since he states as a fact that the right ovary is responsible for the male ova and the left ovary for the female ova, and that ovulation is an alternating process involving the right ovary one month and the left ovary the next month. Thus, by a reasoning of unusual proportions, the Gordian knot of sex determination, the tightest little knot that has ever baffled the combined ingenuity of many master-minds, is severed, and now the glad tidings may be sent forth that by careful watching on the part of the woman, she may decree whether her future child shall be a male or a female.

As is usual in books that hitch their theories to the stars, there is an entire absence of statistics; and while we do not always pin our faith on statistics, since only too often our credulity has been outraged by their gross exaggeration, they nevertheless mean that the writer is not averse from substantiating his theories by the weight of unyielding facts. Nor does Mr. Dawson take up what should prove of interest to him in his present state of receptivity—the sex of the child in those cases in

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\*Causes déterminant les sexes. Docteur Billon (Marseille Médical).



which an operation has caused the removal of either ovary. Surely, considering the number of times one or the other ovary has been removed, it would not be an arduous undertaking to solicit information as to the sex of all children subsequent to an operation; and armed with these data the author might append an epilogue to the present volume: in prose, in case the theories expounded were a bit in advance of healthy reasoning, but in poetry, as illustrated in its highest flights, in case any or all of the prematurity was but the forevisioning of a prescient scientist.

That a spermatozoon should be deprived of all right to interfere in the matter of heredity, is an unkind attack on its intrinsic qualities; and by limiting it to the rather inferior work as a means of impregnation, is denying it the sort of spirituality we have always associated with it, without delving deep into the subject. Even though future investigations confirm Mr. Dawson's theory that a spermatozoon does not determine the sex of the child, we intend to be firm in our belief that some of the hereditary qualities emanate from it; otherwise we shall have to give up all those brilliant ideas with which Professor Karl Pearson\* has regaled us of late. And now that we are stepping briskly along the highway that is paved with all the promises and good intents of the science of Eugenics, we do not intend to recede, just because a medical philosopher of doubtful reasoning powers has seen fit to decry the high purport of a spermatozoon and fulsomely praise the ovaries.

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### THE WORLD'S LANGUAGE.

A problem of great perplexity is the matter—and now we are voicing the opinion of many educators—of arriving, before long, at definite results as to what living or artificial language shall best meet the demands of the many, so that not only a complete understanding among the various peoples shall obtain, but a peace of mind be effected that shall carry with it all the benefits which at present are sadly lacking when polyglot conversations of irritating characteristics are executed. International congresses which convene with a regularity that speaks well for the patience of humankind, since they are witnesses to the great disadvantages accruing from a scientific paper but half understood, are bodies of earnest men who deserve considerable commiseration; and were it in the power of any sublunary being to mend matters, by making of one or another of the living languages the effective and universal medium by

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\*The Scope and Importance to the State of the Science of National Eugenics. By Karl Pearson, F. R. S. London: Dulau & Co.

which advocates of scientific thought could easily convey to others all the nuances of their individual mental status, a decided step in advance would be gained. That this is almost impossible is well illustrated in an article from the pen of Professor Albert Schinz,\* in which he shows quite conclusively that even so virile a language as English would be ineffective, were it possible to make it universal. And since it is a fact that as yet only a minority among us have succumbed to the blandishments of Esperanto, despite its many advertised fascinations, the subject of the impossibility of making any living language the universal means of understandable thought, is fraught with enough interest to arrest all those men, be they medical or otherwise, who, having shivered or fevered through the exasperating period occupied by the reading of a paper in a foreign language, have had the selfish utopian dream of foreseeing the world at peace one day, thanks to the introduction of their own special language.

A language is so decidedly part and parcel of one's nationality, that to displace it by another, though this other may have some advantages as regards simplicity of orthography and grammar, is taking but small account of an individual's predilections. But aside from what the individual might think, or what his deep-rooted objections to the acquirement of a new language might be, would the language itself retain its virility, if bandied about by all sorts of people with but small respect for its intrinsic beauties? Would it be capable still, after some years of hard usage, to express the fine shadings, without which its value is greatly curtailed? We doubt it, and that Professor Schinz has drawn attention to these vital points should be momentous enough to call a halt to our enthusiastic advocacy of a universal language. Again, would it be possible for a dyed-in-the-wool Russian, Hungarian or Bohemian, to make his thought clear in a language with which he has no sympathy, but which he acquires because he was democratic enough to ally himself with the Universal Brotherhood, which forges its links out of such perdurable metal that much is made of intelligibility of thought? And even in a scientific paper, national traits are betrayed and virility of language counts for something. Were it otherwise, some of the subjects would not be hearkened to for a minute; but being bolstered, for the time being, by an enthusiasm that is unmistakably national and therefore closely allied to the speech in which they were written, a certain attraction is lent them. An embryonic scientific thought, exploited for the first time in a medical congress, would be sadly handicapped, if the invigorating language which is native to the speaker, were replaced by

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\*Will English be the International Language?—*North American Review* for May.



a colorless linguistic medium utterly out of keeping with the nationalism of the soil from which it grew. The history of all scientific meetings has proved that new ideas are bound to meet with considerable opposition; but though a counter feeling does prevail, there is never a complete routing of a scientific discoverer or iconoclast, if the shaft is driven home. And to effect this, one's own language is the best, for after speaking it for years it is an integral part of one, and is the only bulwark that stands between the life of a new measure and its devastation, as encouraged by opponents.

Of course, all this would not work weal for those whose limited knowledge of the intricacies of a foreign tongue preclude a full enjoyment of a paper. But observation has taught us that even at medical congresses, the spirit is not all science, and that occasionally relaxation is not an ungrateful adjunct. How to achieve the latter, while the seething currents are plashed by the gutters of some enthusiastic member of the Slavonic race, needs no comment here, for various members are quite adept in passing into a state politely known as a siesta, but which a less kindly critic than the writer of these lines would call a long sleep. But even though this untoward symptom of inattention obtains, there is no reason to suppose that these same men would lend undivided attention to successive papers, were they in their own tongue. To contend that one language-for-all would lighten our labors, when we attend medical congresses as self-appointed delegates, is a very superficial criticism of the situation; for to try to digest dozens of papers in the space of a week, though they be written in a language that is native to the listener, is really beyond the capacity of man. Hence—and here observation is again at our back to guide us on safe ground—the scientific paper droned forth in the gutters of the Slav, is an assuagement greatly to be desired by many tired brains.

The matter of language is really occupying too much of the world's attention to-day. It is being discussed by the visionary enthusiasts,—the ideologues of the artificial languages, as if it were a thing of so little importance, that by a slight movement of the wand which each Esperantist is supposed to hold, it would fall to the ground like an old garment. In fact, so little is thought of the importance of preserving all the beauties of one's own language that even writers, who are not bitten with the disease of evolving a new language, are harsh in their criticism of the worthlessness of their mother-tongue. To cite but one instance, we have before us a book by Ralcy Husted Bell entitled "The Changing Values of English Speech," in which there is much that is closely related to what might be called the Higher Criticism of Living Languages, for this author thinks that the English language has stopped growing, is

"dead," in fact, and "probably reached its perfection several centuries ago." But though this is a scathing view, there are others whose beliefs are just as strong in an opposite direction. Among these, none better expresses just what a language ought to stand for, than does Professor Schinz, for he gets at the root of the matter by recalling to our defective memory the great intrinsic merits of the prominent living languages. "Why do old English, old German, old French appear so delightfully picturesque and graceful and crisp?" he asks; and his answer is no less effective: "Because they were more concentrated languages than our present forms." And this concentration, whether evidenced in a paper read before a literary society or a congress composed only of doctors, cannot preserve its integrity, if it is constantly subjected to the disastrous inroads arising from an unintelligent attitude, on the part of those who should think well enough of their mother-tongue, to recognize the puerility and inutility of substituting, either an artificial or another living language, for their own.

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#### THE TRAGI-COMEDY OF CHRISTIAN SCIENCE.

In the comedy "T'en as un œil," which scored a success in Paris in 1903, a suckling influenced by advanced ideas derived from a foundling conducted on radical lines, refuses to take the right breast because in his opinion only the left breast is imbued with the necessary radicalism! In like manner, after reading the lucubrations which the facile pen of the progenitress of "Christian Science" has launched into the world of religious healing, we feel some justification in refusing all sustenance from a source that makes light of all the verities of the science of medicine, and is even more reactionary than the right breast which filled the advanced Paris child with loathing. This attitude is certainly one of narrowing latitude and might be the subject of scathing rebuke on the ground of a deprecatory stubbornness; but after a course in the tenets of physiology and pathology, the medical mind is really not in an ideal receptive mood to make much of such abstruse diagnoses as "bowel troubles," "female troubles," "kidney troubles," "heart troubles" and "rheumatic troubles," to mention only a few out the vast congeries of "Christian Science" disturbances. Of course, the defects of an early medical education, achieved before the glamor of this extraordinary philosophy dulled the special sense of sight to the gropings of the staunch advocates of correct diagnosis, may have something to do with the failure of a complete realization of the vast benefits that are ours for the asking; but trained as we are to doubt, where a grave disorder is lightly passed over, and receives only the dubious honor of being



named "a trouble," harsh criticism of our front ought to be withheld from us. That we are not completely isolated in our position as con-tempers of the palpable frauds of this modern vagary, the scientific criticism of the day attests; and in no instance have all its weaknesses been better exposed than in the recently published "Faith and Works of Christian Science" by the author of that unforgettable book, "Confessio Medici."

Mrs. Blake, the wife of William Blake, the poet, once said to him: "You know, dear, the first time you saw God was when you were four years old, and he put his head to the window and set you a-screaming;" and if Mrs. Blake were with us now and were a "Christian Scientist," as we feel she would be, almost the same words would be applicable to Mrs. Eddy, for according to her own account in "Retrospection and Introspection," she heard repeatedly, when a child of eight years old, a mysterious voice calling her distinctly by name—Mary, Mary, Mary—three times, in an ascending scale. What else was there for her to do after so extraordinary a performance, than to feel that her duties lay outside the monotonous grind of daily life,—in that supernal atmosphere which is the only medium for a religious healer with aspirations after a saintship. There have been other instances in history where the Mysterious Voice has been the first intimation that the recipient of this special favor had been singled out for an exalted purpose; but others were not so wise as the head of the "Christian Science" church, for they turned their backs on mundane matters in their zeal to further their sublime emotions. Modernity has never forsaken Mrs. Eddy—the modernity that keeps a watchful eye on how best to acquire the wherewithal to prevent the pressure of real troubles in the sear leaf; and from the start of her career as healer she has lavished her gifts on all who were willing to compensate her for a "science" untaught of man. As she aptly puts it in "Retrospection and Introspection": "When God impelled me to set a price on my instruction in Christian Science Mind-healing, I could think of no financial equivalent for an impartation of a knowledge of that divine power which heals; but I was led to name three hundred dollars. \* \* \* God has since shown me, in multitudinous ways, the wisdom of this decision."

The medical schools throughout the country which are not abashed at present to receive large sums for medical instruction, might profit by closely studying the business methods of this modern mystagogue; for though in the beginning she charged only \$100 for twelve lessons, she later raised the price to \$300, and when she felt that her genius was at its apogee, reduced the number of the lessons to seven, without any reduction in the price. Of course, what Mrs. Eddy so kindly taught in the

seven lessons, covered more than a complete knowledge of what is passed on to a medical student during his four years; for each lesson was the sort that would require an ordinary medical student an eon or two to acquire, since it was brimful of all the unearthly things of which Mortal Mind cannot conceive. Nevertheless we feel that the pedagogic performances of the head of the "Christian Science" church were quite lax in one respect—a disregard of technicalities when describing distinct pathological changes affecting the various organs of the body. Hence the present day "Christian Science" characterization of all the ills of man as Troubles.



## OPINION AND CRITICISM.

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### THE REHABILITATION OF BEER AS A DIET.

Agitations against certain articles of diet, led by enthusiasts who reap the benefits of an ephemeral fame on account of an uncompromising attitude, are only too often the outcome of the mistaken and egoistic idea that a prejudice is justification enough for extreme denunciation. Just as in this little world of ours, which revolves so noisily on its axis as judged by the fustian and blatancy of many of the little minds, mankind is uncritically divided into glorious heroes and black villains, so certain food stuffs are classed in the same enlightened and illuminating manner. But the wrangling on the subject of what people really ought to eat to propitiate their interfering friends and retain their status as sane entities, is as nothing compared with the fulminations liberally strewn about in the four quarters of the earth, when the subject of what one ought to drink is dragged out to be mauled into some semblance of a spanked and dutiful child. At no time in the history of man has there been greater animus expressed against liquor than at present; and so forbidding are the skies when the welkin rings with the daily secretion of the bitterness of narrow thought, that many of us are compelled to abide by the decisions of befogged intelligences. But a welcome turn to all this monotony has been granted us at last, for the Special Commission on Beer, appointed by the English Parliament, has reported that beer may be "eaten" without any dire effects to the organism of the unwary.

Of course American beer is not exactly the counterpart of the English product, but the decision arrived at in England is, notwithstanding this, of some value to us; and that its echoes have already reached us was evidenced at the brewers' convention at Atlantic City a few weeks ago. The decisions of a lay convention cannot be of much moment to medical men, since they are undoubtedly influenced, more or less, by a commercial *arrière-pensée*; nevertheless, in this instance, the cue for the exploitation of American beer came from a most excellent source, and as such should merit more attention than mere passing notice. The English Commission worked on the matter for months, and its judgment that beer is a nutrient, that should not be overlooked by the advanced lights in the medical profession, will be welcome to all those hitherto cowed spirits amongst us, who were but weak-kneed and lackadaisical Davids in their warfare against the Goliath of a mighty Puritanism.

An important matter to consider is whether or not American beer has as yet reached the stage of perfection which would make it an excellent adjunct to other nutritious foods. What percentage of alcohol

does it contain? and if this is arbitrary, would it not be well for brewers to evolve a standard that would place the American product in the same class with German beer, which is almost non-alcoholic? In the interests of their now somewhat new mission as purveyors of a nutrient that deserves an honorable position in our dietary list, this ought to be done, and the sooner it is effected, the less will be the prejudice. But even as American beer is now made, it is wrong to class it with whisky; for this sort of attitude betrays an ignorance of its constituent parts, and a stubbornness that refuses to be convinced of the wrong of a wilfulness to further falsehood. The manner in which Commissions go about an investigation are not always in the interests of truth; but no such charge as this can be brought against the English Commission, which by its thoroughness has freed a much maligned article of diet, from the choking tares of a prejudice imbibed by only too many from the inflammable literature of frenetic and misguided pseudo-sociologists.

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#### LITERARY NOTE.

AMONG the new books published by Masson et Cie, none is of greater importance than the "Compendium of Exotic Pathology" (*Précis de Pathologie Exotique*) by Drs. Jeanselme and E. Rist. In the past twenty-five years, no branch of medicine has made such rapid strides as has this special branch, for the reason that scarcely a year passes without some gain in our knowledge due to improved scientific methods. This work is not only for doctors in the Colonial Medical Service, but for all who are interested in medicine, *per se*, and who are desirous of knowing the medical advances which no educated doctor can ignore. The authors, by reason of their previous studies and their long sojourn in the Far East and in Africa, are in a position to write authoritatively; and while insisting on an appreciation of the pathogenic and etiologic factors, on the part of others besides the doctor who is moved only by scientific curiosity, they nevertheless give their best thought to the study of the clinical aspect of tropical diseases and the best diagnostic methods. In regard to the latter, they show the important part played by laboratory experiments, and describe the technique with great clarity, selecting by preference the simplest methods, with a special appeal to the general practitioner who perforce must content himself with but a rudimentary knowledge of the matter. The prophylaxis and therapeutics of tropical diseases, matters which have recently achieved so brilliant a success, receive in this practical and useful work, the minutest attention. But above all other considerations the authors are to be congratulated for not indulging in that hopelessly uninteresting style of writing which is only too often the greatest blemish on a scientific work.



# ORIGINAL ARTICLES.

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## SURGERY OF KIDNEY STONE.

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In the course of my daily work, especially in private practice, I have been impressed, particularly of late, by the frequency of serious disaster of the kidney due to stone. When I recall how important a part in surgery the treatment of stone in the bladder was in the '70's, I am impressed more deeply than ever by the importance of stone in the kidney. The ordinary stone, excluding the phosphatic deposits of chronic cystitis, affected, as a rule, only the bladder. It was comparatively infrequent; and when operation, through aseptic methods, became safe, cutting, either through the perineum or above the pubes, became safe.

Stone in the kidney is a very different matter. The situation—in the heart of a vital organ, and that organ itself in, so to speak, the heart of the body—makes surgery of the kidney of the highest importance. As compared with stone in the bladder, its occurrence seems extremely frequent.

It is not for the abdominal surgeon to say how frequent renal stone is in general, when his opinion is based only upon the frequency of its occurrence in his practice. He can say, as I do, that it is a common occurrence in abdominal surgery to meet with cases in which stone in the kidney is at least suspected; and if, in his total of abdominal operations, 5 to 10 per cent. are for kidney or ureteral stone, it is undoubtedly true that that percentage is due to his particular line of practice or experience or reputation. Be that as it may, cases of renal stone are coming to me now in such numbers that I cannot but be impressed by their frequency.

In bringing up this subject I am influenced chiefly by the fact that many stones in one kidney or both are overlooked through error in diagnosis; and that slowly, perhaps, but surely, changes, which in a few years may prove destructive to kidney, health, or even life itself, may take place.

At a recent autopsy I saw healed tuberculosis of the right kidney. The records contained nothing that would suggest in the history, early or late, the presence of a renal tuberculosis. The pathologist, in answer to my query, said: "Oh, yes; healed renal tuberculosis is more common

that you surgeons appreciate"—a fact very interesting to me. But, so far as I know, this particular healed tuberculosis was not used at all to find out just what clinical evidence there had been of a tuberculosis which eventually healed. The patient could only be questioned, and no one took any special interest in the case except myself.

In spite of all the observations at the autopsy room and at the operating table, I feel that our knowledge of renal stone is very incomplete and inadequate from the point of view of prognosis.

And prognosis, as I have said repeatedly in connection with disease, both medical and surgical, is the thing about which we know least where we ought to know most. Take a case of acute appendicitis—have we not demonstrated beyond dispute that the prognosis cannot be accurately told in any case, and has not the medical and surgical world become convinced, therefore, that the only safe course is earliest possible operation? And in gallstone disease, is not an accurate prognosis the most important fact of all, for accuracy in prognosis depends upon accuracy in diagnosis, and especially upon accuracy in deducing from symptoms the real pathological lesion?

Furthermore, in appendicitis, gallstones, and in all diseases demanding surgical operation, does not prognosis depend upon such things as the operator's skill and experience, the environment of the patient, his powers of resistance, peculiarity of lesion, and complications in other organs? I am convinced that all these questions should be answered affirmatively.

Indeed, it is safe to say that prognosis is practically impossible of accurate formulation for one reason, if for no other—that no two cases are exactly alike. No two cases of gallstones were ever exactly alike, considering all the anatomical areas involved. Taking together the chief organs of life—thoracic and abdominal—the number of combinations which they permit, of disease and of health, is really infinite. So I repeat, in establishing the prognosis, say of gallstone disease, accuracy is impossible. One may say that the combined experience of many observers shows that the prognosis is, on the whole, good, but no man can predict that in the given case the prognosis is surely good, for that case may be the very one in which the combination is bad, and in which the fatal result is going to take place.

In the surgery of kidney stone our experience, as compared with gallstones, is small. But is the number so very small? How do we know that renal stone is not much more frequent than we have been led to expect? We do not know, except by the findings of large numbers of autopsies, as we do in estimating the frequency of gallstones. But I believe the number to be much greater than we realize, as my own recent experiences tend to prove. Renal stone often exists without our suspecting it. To illustrate, for example, the presence of unsuspected renal stone, which caused sudden and unexpected death, here is the case of Mr. K., upon whom I operated for stone in the bladder way back in the



'80's. This man died of suppression of urine in 1907—suppression of urine dependent upon stone in the kidney! Three renal calculi were discovered at the autopsy, and no one suspected in the least their presence. Had no autopsy been performed, death would probably have been attributed to acute Bright's disease.

Is stone an incident in the course of renal disease? Whatever the facts may prove to be, it seems to me highly probable that renal stone is an incident in the course of anomalies of renal secretion.

Once a tendency, always a tendency, is true in many human things. Once a tendency to tuberculosis, always such a tendency. Once a tendency to gallstones, always a tendency, I was going to say, but here we surely have a brilliant exception; for if clinical evidence has convinced me fully of one truth, that truth is that the thorough and complete removal of gallstones is permanently curative, for the operation not only removes from their nidus the gallstones themselves, but it frees that area from the local causes of gallstones—microorganisms. Is this likely to be so in the case of renal stone? To apply the same reasoning that we apply to gallstones, we must assume, first, that renal stones form upon bacteria; secondly, that the operation removes them all; thirdly, that, with the stones, all bacterial elements are thoroughly and forever removed.

But it has always seemed to me in gall-bladder surgery that there are bacterial elements unavoidably remaining in the gall-bladder and in the ducts. When the surgeon's incision into the biliary tract is closed so as completely to prevent the passage of fluids—whether bile or pus—these fluids must remain in the biliary tract, whence they can be thoroughly removed only by the biliary stream. Now, it is against reason that the final closure of the wound, say in the gall-bladder, leaves on the gall-bladder side of the sinus a perfectly normal, healed surface without microorganisms of any kind, especially after operations upon infected gall-bladders. And so in renal surgery, it is beyond belief that the moment the wound ceases to discharge urine—when a kidney is cut open or resected—no microscopic particles remain in the urinary tract to become the nucleus for future deposits.

Such a conviction, even if without sound foundation in fact, seems to me worthy of consideration as explaining the re-formation of calculi in the affected ureter or in the bladder; but it does not explain their formation in the unaffected kidney.

We must, I think, come to the theory that I have already mentioned, namely, that renal stones are "an incident in the course of anomalies of renal secretion"—the result of a tendency of the urine toward the formation of crystalline aggregations while it is yet in the urinary passages.

- The formation of urinary stones upon microorganisms is a very attractive theory, and one that should be capable of easy demonstration.

The first fact to be considered is that stone may exist in the kidney without arousing any suspicion on the part of the patient. In this respect renal stone is very much like biliary. It is doubtless true that renal stone, like other stones, causes little if any trouble until it either gets impacted, and thus excites duct spasm, or until its rough surface causes an ulceration with infection. I do not refer at this time to those calculi which are deposited in infected urines, but rather to the stones which crystallize from an excess of uric acid or oxalate of lime. There are no symptoms in the patient's feelings calling attention to the kidney, either in his comfort or want of it, or in the urine as he is used to observing it as normal—no pain or discomfort, no blood or pus.

The renal stone may exist, as I say, without being suspected; it may be contained in the kidney without being perceived even when the kidney is actually in the surgeon's hands. I have examined several thousand kidneys with my hand in the abdomen. I have detected what seemed to me distinct abnormalities; but I have never, except when the diagnosis had been made or suspected, felt a stone in the kidney in the course of my usual routine examination of the abdominal viscera. A stone may therefore exist in the kidney, not only without arousing any suspicion on the part of the patient, but without being detected by the surgeon with the kidney actually in his hands.

In a recent case I found a stone in the left ureter only because the *x*-ray in the hands of a skilful man, Dr. Percy Brown—with certain significant symptoms—suggested the possibility of a stone there. I even gave up the search, and attributed the symptoms to an abnormal appendix, until I happened to strike upon the calculus by renewed search and by exposing the ureter.

In exploring the biliary tract with the fingers in the course of abdominal operations I find calculi very frequently in the gall-bladder and cystic duct. Sometimes I have found one or more in the common duct, and that, too, with no history of biliary colic or other discomfort.

But, as I say, such a discovery in the genito-urinary tract, especially in the kidney or ureter, is in my experience most unusual. Even when there is a strong suspicion of stone, the surgeon is only too likely to be unable to feel that stone.

It would seem, from these facts, that stone in the kidney or ureter is very infrequent, but such a deduction is not warranted. Judging from the number of cases in which the question of renal stone arises, the number is by no means inconsiderable.

Upon the cadaver it is difficult to detect differences in the health of kidneys by the sense of touch alone. Indeed, I do not know that the pathologist ever makes such observations. The surgeon must depend for his diagnosis wholly upon the sense of touch.

Palpation of the abdominal viscera at the autopsy table, through an incision admitting only the hand, is an extremely useful means of teaching important facts of palpable disease. In many diseases and in



many organs the evidence is about as good upon the cadaver as upon the living body; but the solidification of the paranephric fat interferes seriously with palpation of the kidney. I am not sure that the cold kidney is not very different from the warm one. In autopsies made shortly after death the changes dependent upon cold would not interfere with a palpation closely resembling that during life.

The surgeon who operates within the abdomen will quickly see the need of recognizing, beyond mistake, states of health and disease in the abdominal viscera. However much he may be disinclined to believe what I am here saying, let him be confronted by a tumor of the right kidney and a doubtful efficiency of the left, and he will be forced to admit the importance, not to say the indispensability, of being able to recognize with the finger tips not only the nature of the right kidney tumor—whether demanding nephrectomy or nephrotomy—but the efficiency of the left kidney—whether it is able to carry on an adequate elimination of urine in case of nephrectomy of the right.

Such a question may arise any day, and the surgeon will not always be able to rely upon ureteral catheterization; indeed, he should never rely absolutely upon it.

Take a recent illustrative case. The diagnosis before exploration is not absolutely positive, though it favors renal stone. The symptoms have been pain in the region of the right kidney, and there has been some evidence of renal irritation, as shown by the examination of the urine. The x-rays have not been positive.

The surgeon exposes the right kidney, but is unable, by palpating it, to feel a stone. He next separates the kidney from its attachments, so that he can get in his hand—fingers on one side and thumb on the other. He can compress the renal pelvis between the thumb and fingers in such a way that he could detect a pin head. He feels nothing abnormal either in the pelvis or in the upper portion of the ureter. He then palpates the renal parenchyma, and feels nothing wrong there.

He can be sure that no stone is present in the ureter or renal pelvis, but can he be sure there is none in the depths of the kidney tissue? He certainly cannot. To make as sure as possible, he delivers the kidney as far as practicable, so that he can take it in both hands. He palpates in every direction, and still feels nothing. Is he still absolutely sure? By no means. What next?

If one portion of the kidney seems thicker than another, he may probe this area with a fine, sharp needle. He encounters nothing. What next? Nothing remains but to cut directly into the renal pelvis, unless the surgeon is to abandon the operation as a fruitless exploration. Suppose he cuts into the pelvis through the parenchyma? He subjects his patient to a bloody, formidable, and but too often useless procedure.

I have been in this position a few times and have seen others in it. I have been fortunate in not losing patients after so severe an operation, but I have seen a strong young fellow die miserably as the result of it

at the hands of a skilful operator; and my next case may prove as unsuccessful.

But what is the surgeon to do under such circumstances, when the proper course to follow depends entirely upon the reading of his finger tips? Except for the interpretation of brain conditions by the sense of touch, I know of no anatomical changes so hard to recognize by the sense of touch as those of the kidney; and cerebral conditions which for recognition depend upon such gross observations as those of the exploring finger are indeed discouraging, and should hardly be mentioned in comparison with diseases of an organ like the kidney.

In many, probably in most, renal cases the surgeon will be able to tell just what he feels. Take, for example, a lobulated kidney, three or four times as large as normal, bulging with fluid, in which large stones can be felt. Such a kidney is the seat of pyonephrosis, dependent upon stones or causing stones. But may it not be some other fluid? My last case felt like an abscess of the kidney with stone. It proved to be stone in a bloody cyst. In this very case the question of extirpating the kidney—it was the right—depended upon the diagnosis—whether tuberculosis, simple abscess, hemorrhage, or neoplasm, and especially upon the condition of the left kidney, which seemed to me small and hard. I do not care what the catheterization might have shown in this case, it would never in the world have given such valuable evidence as I got through a median incision with palpation of each kidney—an examination in which every assistant took part. We were able to say that there was something in the right kidney that demanded exploration, and that the condition of the left made extirpation of that right kidney inadmissible unless absolutely unavoidable. (*Office Records*, vol. lxxviii, p. 79.)

I exposed the kidney through my muscle-splitting method, delivered it, aspirated it, and finally resected it, saving three-quarters of its bulk in perfectly healthy renal tissue. And the patient made a good recovery.

The exploration of the kidneys by the hand is, in my judgment, indispensable in renal surgery. A median incision which makes such an investigation possible is, I believe, fraught with as little danger as ureteral catheterization, especially in the male, and adds information of the utmost value which can be obtained in no other way.

In most cases the diagnosis is positive enough to require but one cut—that over the affected kidney—so that the median incision is unnecessary. When the kidney affected is doubtful, I take the median line first, and the second cut, if any, may then be made intelligently.

The first thing, then, the most important that the surgeon can bring to the operating-table in kidney cases, is a good working knowledge of the anatomical conditions of the living kidney, as interpreted, first, by the sense of touch; and secondly, by the sense of touch and sight.

The kidney is a difficult organ to get a clear idea of by the sense of touch alone; yet one can detect any marked changes in size, situation,



and consistency. To detect a stone in the kidney is, as I say, a measure of great difficulty and uncertainty.

A curious condition I have lately found in which there seems to be an enlargement of the renal pelvis on both sides. I was not sure that there was a hydronephrosis, and yet the pelvis was certainly much larger, broader and longer than normal. The finger tips could be pressed into the region of the calices, and the renal parenchyma seemed less bulky than usual. There were no pathological changes in the urine, and there were no symptoms. I noted these variations from the usual with considerable interest.

In pathological cases the interpretation of enlargements is difficult. One finds in pyonephrosis sometimes an enormously enlarged kidney with sacculations containing pus. The whole kidney is a bag of fluid of irregular outline; a sacculated, purulent organ, in the contents of which one sometimes can detect, with the fingers, calcareous masses. Palpation of the ureter emerging from a diseased kidney is impracticable when the ureter is in health. A ureter perceptible to the fingers is an unhealthy ureter, and is usually tuberculous.

It is not my intention at this time, however, to go into details of abnormal anatomical appearances as demonstrated upon the living, except as it elucidates the subject of renal stone.

There are two pathological variations in which palpation is of the utmost importance—the renal stone with trivial change in the renal anatomy and physiology, and the renal stone with complete kidney destruction, whatever the bacterial elements which have accomplished that destruction.

In many cases the secreting power of the kidneys will have been determined with more or less accuracy by means of ureteral catheterization, and yet I have never learned to look upon this manner of investigation with absolute confidence, nor as a means without considerable danger.

The urine examination will doubtless have settled the question of renal tuberculosis, so that the surgeon will have much information about the kidney when he first reaches it with his fingers. Relying on the fingers alone, no examiner will be able to express a positive opinion as to the exact nature of the process of renal destruction, though he will get a very good idea of its extent. He will be able, in suspected stone, to tell when he really *does* feel the stone, but he will not be so certain when he does *not* feel it. It is much easier to rely upon a positive feel that there is a stone than a positive feel that there is not. When a surgeon once gets the sensation of renal stone through the renal pelvis or parenchyma, he knows it just as positively as he knows when he gets the feel of a gallstone. When he thinks he may be feeling one, but is not sure, he becomes absolutely uncertain as to the reliability of the finger tips.

Hence the grave uncertainty with which the surgeon cannot but be depressed when the history, clinical symptoms and x-rays all point to a

stone which he cannot be sure he feels, and to demonstrate the presence or absence of which requires a bloody dissection of much danger.

This investigation of the comparatively normal kidney containing a possible stone is a matter of the first magnitude in renal surgery. The operator must, by thousands of observations, accustom himself to the feel of the normal kidney in life. There is no other way of educating the touch, and he who practices this touch the most faithfully and frequently will be the one to interpret most accurately and fearlessly the reading of his finger tips.

I am surprised every day that so little attention is paid by operators to the education of the touch in this regard. We all know perfectly well that the guide to the lesion and the indications for operative measures are what the fingers feel, aided occasionally by other means. All regions not easily accessible to the eye must be studied with the fingers. It is safe to say that very few abdominal tumors are diagnosed by the sense of sight. Even if they are large and visible, as ovarian cysts are, the diagnosis that really influences the operator is that of parts which cannot be seen. Three cases within a week have shown this: Mrs. W. (vol. lxxix, p. 49), with a large benign tumor presenting, had cancer of the pelvic portion; Mrs. H., with a multilocular ovarian cyst, had cancer infiltrating the depths of the pelvis, invisible and inaccessible without removal of the bulk of the tumor; and Mrs. M. (vol. lxxviii, p. 153), with a forty-pound multilocular cyst which had unknown possibilities of malignancy until after thorough extirpation.

Renal surgery is an important part of abdominal surgery, and the surgeon whose experience is large is best educated to perform it—not in the narrow specialty of genito-urinary surgery, but through the broad experience, first, of general surgery, and later of abdominal surgery.

In no other way can the surgeon take that broad view of renal lesions and their appropriate treatment. General surgery, with wide experience in medical diagnosis, will best give, I am convinced, that broad grasp of any special abdominal disease which will enable the surgeon to apply a wise course of treatment, whether conservative or radical.

And renal surgery—as Fenger said—should be whenever possible radically conservative. Risks are justifiable to save the precious secreting tissues of the kidney when risks are not justifiable to destroy it.

When the surgeon examines with his fingers kidneys which show unmistakable changes in shape, size and consistency, he must determine what the lesion is, what it means to the patient, and what it demands in order that the patient's health may be, with the least risk, permanently restored. When there is little deviation from the normal found, he will have had perhaps evidence more or less reliable from chemistry, bacteriology, x-rays and ureteral segregation.

I admit my personal feeling of responsibility when confronted by the problem of anatomical diagnosis on the living patient, with kidney fully exposed to manual palpation.



What, in advanced cases of renal stone, does the surgeon feel; what evidence indicates nephrotomy; what nephrectomy; what no operation?

This question expresses the gist of the whole matter. Would that it could be answered as easily as it is asked.

The surgeon must not only ascertain accurately with his fingers the extent and nature of the pathological condition, but he must, upon that demonstration, base an accurate prognosis of the future of that kidney under efforts to conserve what of it is good, as well as the prognosis of the patient under the proposed local operation. And furthermore (as I say elsewhere), he must lay down the prognosis as influenced by his examination of the other kidney, plus the probable effect of the proposed operation.

The advanced changes by which the kidney is practically destroyed, or by which enough of valuable tissue is left to make conservatism worth while, cannot always be told with accuracy, even with the kidney in the hands. I regard a sacculated kidney as practically worthless; a kidney with a single sac, though it occupies a third or a half of the kidney body, as worth trying to save, if there is reason to think that the pelvis and ureter are free, as determined by ureteral investigation. Yet it will not do to assume, if such a kidney is giving no urine through the catheter or the ureter, that it is incapable of valuable work and more or less restoration of function, for the ureter, even if blocked, may by drainage become patent.

The trend of thought and study should be toward conservatism. In trying to explain the loss of renal function, we should make every effort to find an explanation which by conservative operation will permit restoration of that function.

The one thing to remember in palpating the kidney just prior to operating—especially before doing a destructive operation—is that there is every reason for trying to save the kidney; that the burden of proof is on him who would destroy rather than on him who would conserve so valuable an organ.

Some ten years ago I removed a tumor of the left kidney, after thorough palpation of the right. There was what seemed to me a sarcoma of the left kidney. The right kidney was not exactly normal to the touch, yet it was not especially abnormal. I removed the left kidney, which proved to be a shell of renal tissue—good for work, what there was of it—about a hydronephrosis. The patient died with anuria. Autopsy showed the right kidney to be also a shell, but smaller. It was a case of double hydronephrosis not recognized by the touch. The operation was strongly contraindicated, and was, in fact, fatal because it destroyed just that amount of renal tissue which enabled the patient to live. With both kidneys partly disabled, life was possible; with one kidney removed, life was impossible. Kidney catheterization in skilful hands would, I think, have saved the patient from the error of judgment. This was, however, before the day of ureteral catheterization.

Catheterization of the ureters is a highly specialized procedure, especially in the male; and its value to the surgeon may in a general way be gauged, I think, by the practical use to which one in active surgical practice puts it.

I have observed the methods and the results of cystoscopy and of instrumentation of the ureters in the hands of men who have made special study of the subject, and whose skill is great. My opinion is that this method of investigating the kidney is useful only in the hands of highly trained men who have devoted an enormous amount of time to the practice of its technique, to the recording of their observations, and to the verification of these observations by operations which give abundant opportunity for visual and manual examinations and control.

I cannot but regard even so comparatively simple a measure as cystoscopy, except in the hands of the most highly trained expert, as one which is not only valueless, but actually misleading. Even in the hands of such men, instrumentation of the ureter I believe to be an operation of danger. I do not accept efforts of the very best without reservation; and in every case, no matter what the expert report, I feel constrained to verify with my own hands and eyes the findings of ureteral investigation through the bladder. Right or wrong, through abundant experience with my younger colleagues, who make these examinations with extraordinary skill, I would never remove a kidney without actually feeling of the other one for information which is indispensable, and which can be got in no other way. If, then, I have been brought by abundant experience to this conclusion, and if this conclusion is a sound one, what are we to do about ureteral catheterization and cystoscopy in renal surgery?

Cystoscopy will, with very slight risk, show what is coming from an affected kidney—pus, blood, or nothing at all abnormal; catheterization will tend to show the condition of each kidney;—with the results of manual examination, it may be of great value. A kidney, for example, which the catheterization shows to be healthy, will be proved healthy by the actual feel of it; a kidney which is pronounced normal may be all right and it may not. If the manual examination confirms the instrumental, all is well; if it does not confirm it, let the surgeon be on his guard.

In tubercular cases I would forbid ureteral catheterization under all circumstances.

In considering the symptoms of renal stone, we have, first, the clinical picture of renal colic in which the pain is excruciating—an agony unbearable—the patient writhing in torture, with reflex signs in the expression of the face, the cold perspiration, the cold extremities, the accelerated pulse. In the male there is in many cases retraction of the testicle on the affected side, with pain shooting down into it. Locally there is tenderness, and a soreness that persists some days after the successful passage of the stone to the bladder. I have seen, in addition to these local and classic signs, distention of the abdomen, which, with



tympany and vomiting, has suggested a peritonitis. One must, indeed, look out for a renal cause in some forms of suspected acute general peritonitis.

The urine often contains blood enough to make its presence conspicuous to the eye; if not, the microscope will show red corpuscles and other signs of renal and ureteral irritation. Altogether the picture is clear and unmistakable.

In the present paper I am considering, however, not so much the typical case as those comparatively rare ones in which strongly suggestive histories and signs are wanting. I am considering rather those cases which are diagnosed after a prolonged study suggested only by a suspicion; for many a stone will be found, once the symptoms suggest it—no matter how slightly. Just as the bulk of gallstone surgery comes to-day from histories which even ten years ago would have excited no great comment, so to-day, as I fully believe, the early and brilliant surgery of renal stone is going to depend upon lines of investigation suggested by very indefinite symptoms.

To show you the truth of this prediction, I have only to bring forward cases of renal and ureteral stone treated by appendectomy; cases of renal stone treated for gallstones, for gastric symptoms, for neuralgias, indigestions, intestinal colics. I may be widely prejudiced by the numbers of cases that really have drifted into my office, but when two or three patients have come in a single afternoon, I cannot but wonder whether renal stone is not really a much more frequent disease than we have usually thought.

Renal stone, like gallstones, speaks first to the patient and in a way which may drive him to the physician, or may excite only a shrug of the shoulders and a call for a plaster or some medicine for rheumatism. The first and most important call upon the patient's attention is some attribute of that broad word *pain*.

The pain of renal stone I cannot as yet speak of with that confidence which a vastly larger experience in the diagnosis of gallstones has given me. Nevertheless, I can in a hundred cases, more or less, give you some idea of my meaning when I say that stone in the kidney makes known its presence sooner or later by symptoms of irritation described by the patient in some expression meaning pain. That expression may be a transitory and trivial discomfort, or it may be an unbearable agony; it may be a heavy, solid, unending pain; it may be a nauseating distress. It may increase slowly or quickly to its acme; and it may subside in the same fashion. The acute, excruciating pain of a stone engaged in the ureter may likewise begin instantly and without warning and persist in paroxysms of indescribable severity. The engaged stone not successfully pushed along by ureteral spasm will halt where it becomes first impacted, and the painful and unsuccessful spasm will give way to a dull, deadly grind, which in turn will slowly disappear, with the occasional recurrence of subsiding duct spasm. When the stone is fastened ir-

remediably in its ureteral nidus, its presence will be made manifest by pains of varying character and severity.

Stones in the kidney, according to shape, size, situation, roughness and the like, will in theory at least be responsible for symptoms only when they cause irritation with or without muscular spasm, with or without infection of the urinary tract. I am not considering at present stones which are formed as the result of infected urine, but stones which are sometimes the cause of infected urine. What these symptoms are can be determined only when we know surely that stones are present. We know, of course, that that one great sign is pain of some sort; but what kind of pain to expect with certain shapes, sizes and situations of stones, we do not know; nor can we tell by the kind, situation, extent, persistence, onset, cessation, or other attributes of the pain, just what the shape, size, situation and other attributes of the stone may be.

The signs of irritation, then, are subjective feelings of discomfort, from the most acute and uncontrollable pain to a hardly noticeable malaise.

Constitutional signs may or may not be present. It is safe to say that fever has a grave significance in renal stone, for it means changes of far-reaching importance in the renal pelvis. Fever not only confirms the presence of suspected stone, but it adds the greatest possible weight to the indications for treatment. I was inclined to wait and study out my lesion in a case of renal stone (vol. lxxviii, p. 75) until I found that there was a daily temperature of 102° to 103°. The diagnosis was confirmed by the *x*-rays, but I wished to follow the clinical course for a week or two. There was so much fever that I deemed this course unwise, and advised immediate operation. The patient declined to follow my advice, as he had declined to follow Dr. Jones'. The subsequent history showed spontaneous recovery.

This patient had a severe attack of pain, with rapid pulse and temperature of 102°. He was in bed about twenty-four hours. He has been well ever since he recovered from this attack, which was probably ended by the passing on of the stone.

That there may be a transitory fever which does not have a grave significance, I have no doubt; but the irritation of stones in the renal pelvis means, sooner or later, a destructive pyelitis.

The symptoms of the greatest importance are those found in the urine. The commonest of all is blood, either enough to give a distinct bloody tinge or to fill the slide with red corpuscles. With blood there will be found pus corpuscles, and such crystals as, on the chances, compose the stone—uric acid, oxalate of lime, and the like.

Long before the *x*-ray examination is made, the surgeon's attention will have been attracted to the kidney as the source of renal pain. The *x*-ray will show a stone, surely or doubtfully, if one is present that casts a shadow.



The physical examination in suspected stone will seldom reveal any sign of importance, unless changes have taken place in the kidney which increase greatly its size.

Once the attention has been drawn to the kidney as the source of symptoms, the diagnosis of stone is easy, provided the stone can be shadowed by the *x*-ray. When, for any reason, but chiefly from a composition which throws no *x*-ray shadow, this means of demonstration fails, the diagnosis is not always easy. I have made many correct diagnoses with the aid of the *x*-rays, some in spite of it, and a few against its evidence. It is well, therefore, first to consider the diagnosis of renal stone as if the *x*-rays were unknown.

We must weigh first and most carefully the evidence of pain. While it is true that a stone may exist with little if any pain, yet in the absence of pain no stone would ever be suspected by the patient or by the physician, unless something decidedly abnormal were discovered in the urine. Hematuria would surely attract the patient's attention and bring him to the physician. Any unusual deposit, color, or consistency might start the train of investigation. Whatever the source of the patient's anxiety, the physician, seeking for a cause of urinary changes, would last of all suspect stone were pain absent.

The significance of pain in general affecting the kidney or the ureter, and later perhaps the bladder, lies in its character, its mode of onset, its extent and breadth, its way of stopping, the means necessary to overcome it, its effect upon the patient. The weight to be given pain depends upon the accuracy with which it is observed, the manner in which it is described, and upon the patient's powers of bearing it. Any discomfort persisting in close proximity to a kidney should excite a suspicion of renal stone, especially in the left abdomen, where there is neither appendix nor gall-bladder to confuse.

The only pain which will be characteristic enough to indicate a positive diagnosis will be the awful spasm of renal colic. The less like renal colic the pain, the less significant of stone that pain.

Tenderness is a symptom which confirms the localization of pain. Pain always in the same spot, and tenderness always in the same spot, are two very telling symptoms. Here again is a close analogy between the gall-stone and the renal stone. Tenderness along the course of a ureter is the rule after the passage of a calculus; and when the stone is impacted, the tenderness is at once localized over the spot and remains there. Tenderness is present in all the later organic changes in the kidney due to stone.

The significance of tumor is of the first importance, for with a diagnosis between gall-bladder, kidney and appendix the discrimination will be positive, and usually easy by palpation of the tumor alone. Indeed, so easy is the differentiation that I do not intend to spend much time in considering so plain a subject. Pain, tenderness, perceptible gall-bladder, mean gallstones. Pain, tenderness, tumor of the kidney, mean

kidney stone; the same, with a tumor in the appendix region, means appendicitis.

But does it always mean this? It does not, and it is here where skill in diagnosis, based upon experience, comes in. It is not always possible to tell the difference between a gall-bladder and a kidney, though the problem is usually easy. Nor is it easy always to tell that a tumor in the right iliac fossa is appendix, for it may be an impacted ureteral stone. Nor can you, in acute pain and tenderness, with an apparent gall-bladder enlargement, be always positive that it is not a high appendix. I have seen several errors of this kind, and I urge extreme care in studying the case from all sides, and not being content with a diagnosis based, as I have based it thus far, on pain, tenderness and tumor, without considering other signs.

A diagnosis is, however, strongly indicated by pain, tenderness, a tender and enlarged kidney. Is it strong enough to base a sure indication for operation? It certainly is not, without study of the urine, which under careful examination ought to confirm or deny the opinion of offending renal stone. I regard bloody urine as pathognomonic of renal or ureteral stone, when there has been pain, with or without tenderness in the kidney or along the ureter. If there are only a few red blood corpuscles in the field, I regard the fact as one of great significance. Pus with blood indicates renal irritation but of longer duration, though the pus may come from some part of the urinary tract unaffected by the stone.

In cases of advanced renal destruction, there will be only the abundant pus of pyonephrosis, the diagnosis of which—whether calcareous, tubercular or of other nature—exceeds the scope of this communication. Suffice it to say that the observer will be on the right track when he finds abundant pus with an enlarged kidney, to apply the final and most important test of all—the  $x$ -rays. The diagnostic value of the  $x$ -rays cannot be exaggerated in the great majority of cases. When the  $x$ -rays are in the hands of skillful and experienced Röntgenologists, a positive opinion that stone is present outweighs any amount of negative evidence.

I have not always felt in this way, for in the beginning I was willing to take the chances against the diagnosis, even with positive  $x$ -rays. And I was successful so many times that I began to feel a certain contempt for an  $x$ -ray diagnosis. I have been convinced in the past year that the  $x$ -rays in the hands of the expert are always *just what he says they are*. And when he is positive, I feel positive enough to explore a kidney in which I feel nothing. When the  $x$ -rays are negative, however, and my own diagnosis positive, I am inclined to explore in spite of the findings of the  $x$ -rays, especially in view of the fact that stones of certain composition throw no shadow.

Other things besides the shadowless stone seem to make the  $x$ -rays unreliable—the thickness of the abdominal wall, the condition of the bowels, and the like. My own guide to the interpretation of renal



symptoms, when the  $x$ -rays are doubtful, is the pain, tenderness and urinary signs. When there are signs in the kidney, the diagnosis is all the more positive. And when my diagnosis is so positive as to conflict seriously with the evidence of the  $x$ -rays, there is pretty sure to be a renal stone which does not throw a shadow.

In a word, then, the diagnosis of renal stone rests upon expressions of pain. Pain suggests stone; tenderness makes the suggestion stronger; blood in the urine strengthens the diagnosis enormously; an objective sign like a tumor makes the diagnosis still more sure, though it adds the element of neoplasm, which may obscure the diagnosis or throw it out entirely. Finally, the  $x$ -rays prove the presence of stone by positive evidence, though they cannot disprove it by negative. Ureteral catheterization I should never employ for diagnosis.

The *indications for treatment* in suspected renal stone are, I think, quite clear in the ordinary unilateral case. Indications are based upon the prognosis of renal stone left to itself, and the prognosis of operation for its removal. As I have already said, the future of a kidney containing a stone in its parenchyma, its pelvis, or its ureter, is distinctly serious. No renal stone was ever yet dissolved by medical treatment. Once a stone has begun to crystallize, it is only a question of time when the affected kidney will be totally destroyed; and that destruction, usually slow, will be attended by suffering and disability to the end.

The prognosis of operation for removal is in the beginning good; the earlier the operation, the greater the safety, with this exception, that nephrotomy through young and healthy renal parenchyma is more bloody than that through the shell of a pyonephrosis. Moreover, the other kidney has slowly assumed the duties of double excretion. But the prognosis for cure with a good resulting kidney is infinitely better with the early operation.

Operation is therefore indicated in a hopelessly impacted stone—one too large to pass into the bladder and out through the urethra. The indications are modified by the condition of the other kidney. The question of operating upon a single kidney demands a special consideration beyond the scope of this paper.

Operation is contra-indicated on general principles when there are other serious lesions of heart, lungs or the kidneys themselves. Then "half a loaf is better than no bread"—a livable life is better than no life.

As to surgical treatment, I do not intend to go into operative technique in detail. I have long practiced a method of my own—I do not know that it is original—of approaching the kidney through a muscle-splitting incision in the flank. I am able to separate, and deliver into easy manipulation, the ordinary kidney. I palpate with extreme care, and, if necessary, incise the renal pelvis through the parenchyma. Best of all, I cut directly through the pelvis itself, at its lower edge usually, and explore with eye, finger and instrument the pelvis, calices

and ureter. This cut I close effectively with fine sutures, and the bloodless operation is finished. When pelvic incision is impracticable, I go through the kidney tissue, but I find it fearfully bloody. I close the opening with catgut or leave a drain; or pack with gauze.

The results are overwhelmingly good as to immediate prognosis.

In the treatment of kidneys containing one or more stones the chief question to consider is whether to remove the stone by simple incision, or to resect the kidney, taking out that which is bad and leaving that which is good, or to remove the organ *in toto*, with as much of the ureter as seems affected.

This question, as I have said, is one of interpreting accurately living pathology. Such interpretation is, I repeat, a matter of practice, of examining repeatedly hundreds and thousands of times living kidneys in perfect health, and living kidneys is more or less extensive disease. And when the question has been decided as accurately as it can be while the kidney is still in the body, shall the leaning be toward conservatism or toward radicalism? After removing the stones, shall we remove also the kidney, or that part of it chiefly affected, or shall we give the kidney a chance to heal and to make what contribution it can to the general bodily welfare?

I must admit again my adherence to Fenger's splendid principle of saving, by strenuous effort, all of the "royal tissue" of the kidney possible, and I must add my voice to favor, whenever possible, conservative methods. But, at the same time, I am sure that the decision between destruction and conservatism requires an experience and a knowledge which few possess. At present the discrimination between the pathology which invites conservatism and that which demands destruction is an extremely nice one, beside which the decision for or against extirpation of the gall-bladder is crudeness itself.

I have saved kidneys which ought on the spot to have been sacrificed, and which later I was obliged to sacrifice. One of the patients suffered irreparable damage by delay, the other was no worse for the delay.\* I have in the second case the great satisfaction of knowing that I did my best to save the kidney; but had the patient become generally tuberculous, like the first one, I should have regretted it all my life.

These two cases illustrate, however, the difficulty of the decision. I have several patients now whose kidneys I saved, and whom I am watching with misgivings for signs of additional trouble.

And yet the truth must be that kidneys are capable of conservation; that a half or a third of a kidney must be worth saving; that there is no more need of sacrificing the whole of a kidney when a part only is diseased than there is a similar necessity in any other organ.

My plea is for conservatism when there is a clear pelvis and ureter,

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\*I was mistaken. When this was written she was at death's door, as I supposed, with hopeless tuberculosis; she is now in splendid health.

and when the disease is not tuberculous—provided always that there is enough sound tissue left to make the effort worth while.

The ultimate results are, however, beginning to disquiet me. The chief source of discouragement lies in the recurrence of stone, either in the kidney from which a stone has been removed, or in the second kidney after extirpation of the first. I do not see how we can answer fully and satisfactorily our question as to the frequency and cause of recurrent renal calculi until our experience has been multiplied a thousand fold. For myself, I believe that the coming treatment of renal stone is an immediate removal by operation as little injurious to the renal parenchyma as possible, and by as clean and brief operations as possible.

Whenever the least question arises as to the condition of the other kidney, I believe that the incision should be carried through the peritoneum, so that its condition may be determined by direct palpation.

The deliberate breaking into the peritoneal cavity in an operation which is sure to be accompanied and followed by more or less escape of urine, sometimes septic, has always added, in theory at least, a menace which can easily be avoided by making the operation strictly extraperitoneal. But in nephrectomies I have made it an invariable rule to do this very thing, in order that I might determine beyond the shadow of a doubt the presence and state of health of the other kidney. In many there has been extensive soiling of the field by the pus of the infected renal pelvis, and yet I have seen no cases of general peritonitis follow.

There is much to be learned concerning the surgical diseases of the kidney, and the road for improvement and knowledge lies, as I have here indicated it in renal stone, along the path of the general practitioner into whose hands the earliest symptoms and treatment of the disease is sure to fall.



DIAGNOSIS AND TREATMENT OF EARLY CASES OF  
TUBERCULOSIS.\*

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By LAWRENCE F. FLICK, M. D., of Philadelphia, Pa.

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Tuberculosis is probably always primarily a lymphatic process. As such it cannot be diagnosed except by reaction. Occasionally the lymphatic glands manifest the presence of the tubercle bacillus by enlargement. It is possible, however, that when this happens there is mixed infection and that enlargement occurs only when mixed infection exists.

Diagnosis of tuberculosis of the lymphatic tissue when there is no, or little, enlargement of the glands, is impracticable. The condition is not a disease in the ordinary sense of that word, since it produces no discomfort and no interference with the ordinary pursuits of happiness.

Tuberculosis of the lymphatics may produce changes in physiological function and in the form of the body. These changes do not set up disease, however, in the ordinary sense of that term. Underweight relative to height, hypertrophy of the lymphatic tissue in the mucous membranes, bad muscular development and various nervous symptoms, may be due to it, but the relationship between it and the condition is not recognized.

Immunity, no doubt, frequently develops in these mild forms of tuberculosis and recovery takes place without the process having been discovered. These are the recoveries which leave earmarks found at autopsy upon deaths from other causes and help to swell the number of implantations of tuberculosis usually recorded.

The form of tuberculosis which most concerns the physician, is lung tuberculosis. The lungs are most frequently the seat of the tuberculous process next to the lymphatic system, and usually the first in which invasion of parenchymatous tissue occurs. This is because the lungs are most exposed to implantation when tubercle bacilli pass from the lymph into the blood. The lymph which goes into the blood all passes through the lungs before it goes into any other part of the body and the lungs are apt to strain out the bacilli which may exist in it.

The apices of the lungs, along their posterior pleural border, probably are the primary seat of lung tuberculosis. Probably both sides are implanted at the same time, but usually one side only has a successful growth for the time being. As a rule this is the right side. The first growth is usually very small and does not give rise to symptoms which

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\*Read before Section II. of the International Congress on Tuberculosis in Washington, in October, 1908.

attract attention. This is the real incipient stage of lung tuberculosis, the stage in which it would be most profitable to find the disease. Unfortunately, it is not often found in this stage.

The discovery of tuberculosis in this stage is possible but difficult. The chief obstacle in the way is the general ignorance upon this subject. The people at large have an idea that tuberculosis cannot exist without violent cough, sweats and emaciation and the medical profession, unfortunately, still looks upon everything short of a fatal condition of the lungs as a mere cold. With the mind preoccupied with erroneous ideas, both by the laity and by the profession, tuberculosis of the lungs is not looked for until health is seriously undermined.

For the diagnosis of tuberculosis, when the process is still limited to the outer border of the lung and the infiltration covers a small area, the mind must be alert and follow every clue which arouses suspicion. If a person is in ill health at all, if he deviates from the normal in weight, in color, in strength or in his physiological functions, tuberculosis should be looked for. If the history of exposure to the disease exists, even though the general health be good, it should be looked for. In this connection one should bear in mind that early tuberculosis of the lungs may produce symptoms and may not produce them. If symptoms exist, those symptoms should be followed out until a logical explanation of them has been found. If a history of tuberculosis exists and there are no symptoms, a searching investigation should be made until the existence of the disease can be definitely excluded. In this regard nothing should be taken for granted.

The subjective symptoms in these cases may be an occasional cough, hypersecretion of mucus in the respiratory tract, sometimes loss of appetite, a feeling of malaise, especially in the forenoon, sometimes headache in the afternoon and occasionally a hypersensitiveness of the entire nervous system. There may be alternative vivacity and depression. As objective symptoms, one may find, if the temperature is taken at short intervals during the day, a slight rise of temperature above the normal, possibly reaching 99 and sometimes 100; a pulse above the normal and pupils dilated, perhaps one or both. On physical examination one may find slight broncho-vesicular breathing over the affected area, perhaps merely harsh breathing with a slight pleuritic rub on expiration; some increase in tactile fremitus and vocal resonance, as a rule, but not always; slight bronchophony, and usually impairment of resonance. The impaired note may be merely a deviation from the normal but it is distinctly circumscribed. Physical signs are more apt to be found over the back than over the front, but may also be found above the clavicle and over the clavicle. With a circumscribed lesion of this character, and subjective and objective symptoms corroborating the story told by the lesion, the diagnosis of tuberculosis is justifiable, however improbable it may look otherwise.

Tuberculosis of the lungs of a more advanced character than that just described may still be termed early tuberculosis, both from the viewpoint of time and of prognosis. Infiltration of a whole lobe, with some softening and even with slight cavitation, and infiltration of one lobe with scattered tubercles in another lobe, or even in two other lobes, may be considered early tuberculosis of the lungs from the diagnostic point of view. As a matter of time, tuberculosis of years duration may be called early when the disease is very chronic, because in such cases the natural duration of the disease is many years. As a matter of prognosis, tuberculosis is early so long as recovery can take place without much loss of tissue. It is in this sense that we must speak of early diagnosis of tuberculosis, because it is in this sense only that we can get cases to diagnose. Tuberculosis really does not become a disease until considerable tissue has been invaded, often not until some tissue has been destroyed, and the physician cannot expect to be consulted until it has become a disease.

The only way in which lymphatic tuberculosis and lung tuberculosis in its very beginning are likely to be found is by looking for it in well people who have been exposed to tuberculosis intimately enough to make an implantation possible. The probabilities are that all people who have been exposed in this way have tuberculosis in the lymphatic stage, or the very early lung stage. Search for these kind of cases may be made in dispensaries and occasionally in private practice, but cannot be pursued extensively in private practice in the present state of public enlightenment, inasmuch as to do so would expose the physician to criticism.

Diagnosis of lung tuberculosis, when a whole lobe is involved, is easy enough and failure to make such a diagnosis at the present day lays the physician open to just criticism. The history of the case and the subjective symptoms ought by themselves to enable the physician to make a tentative diagnosis. With a history of exposure to tuberculosis, with malaise, gastric disturbance, pain, some sense of shortness of breath, a sense of physical incapacity, cough, expectoration, chilliness or a sense of heat and loss of appetite, or at least of relish of food with any or all of these symptoms and with absence of normal physical signs over a part of the lungs, unless the symptoms and the condition can be definitely explained in another way, the diagnosis of lung tuberculosis is always justifiable. Absence of tubercle bacilli from the sputum, in such conditions, must not be allowed to weigh too heavily against such a diagnosis.

To the trained expert a physical diagnosis in these cases is easy and can be made positively. When there is mere infiltration and the tubercles are small and scattered, the vesicular breathing over the part will lean very strongly towards the bronchial type, but will be vesicular in character. There is prolongation of the respiratory murmur and usually a slight pleuritic rub showing itself by an expiratory whiff with



the last half of the expiratory movement. There is a slight increase in tactile fremitus, slight increase in vocal resonance, and there may be feeble bronchophony. On percussion there is well defined impairment of resonance but not dulness. The most distinctive feature of these physical signs is their localization. When infiltration is dense and there is secondary infection, with perhaps some pneumonic process, or the tubercles have become very large and have softened, there is bronchial breathing, increased tactile fremitus, marked increased vocal resonance, marked prolongation of the expiratory murmur and dulness on percussion. The dulness may end abruptly or shade off to marked resonance along the border lines between the diseased and healthy lung.

When the process has gone a little further and some of the nodules have softened and been ejected, much of the congestion has disappeared from the lung and the breathing may again have become largely vesicular but usually is somewhat feeble. Tactile fremitus may have become normal again, vocal resonance may have become normal and bronchophony likely has been replaced by whispering pectoriloquy. On percussion a tympanitic quality is found in the note and if the excavation is at all large, localized tympany can be elicited. When the small cavity has dried up completely and the process has taken on a condition of rest, the physical signs in these cases come so near to the normal that the condition is often overlooked. These are the cases in which the general practitioner most frequently fails to make a diagnosis. When an individual with such a condition, however, takes a cold the physical signs take on a character which approaches nearer to the physical signs existing in more acute conditions and this makes the diagnosis easier. Absence of the normal physical signs should always raise the question of tuberculosis in the mind of the examiner, and, if he cannot clear up the case himself, he should send it to someone who has been specially trained for work of this kind.

The treatment of early tuberculosis is simple and fairly accurate. There undoubtedly is a greater tendency to recovery than to destruction. Treatment is a retracing of the steps which led to development.

It is worth while, however, in discussing the treatment of tuberculosis to define at the outset what is meant by recovery. As there is no disease in the ordinary sense of the term before considerable damage has been done, so recovery does not necessarily imply sterility from the tubercle bacillus. It is questionable whether a person who has had an implantation of tuberculosis ordinarily ever gets free from tubercle bacilli. Recovery, therefore, means arrest of the process of destruction and restoration to physiological health, but nothing more. For practical purposes this recovery is as good as any one can wish for. The only drawback to it is that it only lasts so long as the mode of life which brought it about is adhered to. It may be that in recovery before destruction of tissue has taken place the host becomes absolutely free from bacilli, but of this we are not certain. We have reason to believe that

the individual who has made such a recovery earns thereby a fair immunity against subsequent attacks. Our clinical observations on human beings in this regard have been corroborated in a sense by Calmette's and von Behring's experiments on animals.

For recovery from uncomplicated and unmixed lymphatic tuberculosis and the first stage of lung tuberculosis, all that is necessary as a rule is a simple life, properly selected diet, fresh air and such medication as is necessary to restore the human organism to its physiological functions. Where these things have not been departed from and an implantation has taken place by reason of intense exposure to contagion, recovery usually is brought about by nature without intervention and without discovery of the process. People develop tuberculosis when it has been implanted through overwork, dissipation, improper feeding, too much indoor life in badly ventilated rooms and depression from some great disaster, grief or acute disease other than tuberculosis. It is even quite possible that an implantation will not take place in a perfectly healthy normal human being.

For recovery from tuberculosis, when the disease has gone beyond the lymphatics and has invaded considerable lung tissue, with possibly some destruction of tissue and with perhaps even a slight mixed infection, somewhat more heroic measures are necessary. In this stage the human organism has already suffered somewhat in its physiological capacity of fighting disease and making nutrition. There is already a deficit in the bank account. The treatment must seek to re-establish the credit of the human economy. With this object in view consumption of energy must be curtailed and the manufacture of energy must be encouraged.

The treatment of this kind of cases is really the practical problem of the physician because these are the early cases which are likely to consult him and in which he can produce good results. Most of these cases can be restored to a condition of physical health and a complete arrest of the disease.

In proportion as the disease is advanced in the stage of destruction of tissue it becomes necessary to put the patient at rest to economize expenditure of energy. As most patients come to the doctor with a mild attack of mixed infection from a cold, a pneumococcic infection or influenza, it is good practice to send every patient to bed for a week or two as the beginning of the treatment. Complete rest in bed for these cases usually has a marvelous influence for good. The heart is relieved of its excessive burden and the circulation in the lungs at once improves, with abeyance of cough and all the acute symptoms which have brought the patient to the doctor. This rest in bed also facilitates the recovery of lost energy as manifested by lost weight. The patient usually gains considerable weight during this rest. After a few weeks of absolute rest in bed the patient may advantageously be put upon comparative rest, that is, sitting up the greater part of the day and resting in bed during the middle of the day and during long hours over night.

Later still he may be put on light exercise to be gradually increased to the point of endurance of a fair day's manual work. In the latter part of treatment, work or exercise is of just as much importance as is absolute rest in the beginning, a fact which should not be lost sight of by the physician who is looking for good results in the treatment of his cases.

For the manufacture of new energy a carefully selected diet is of much importance. This diet should be varied somewhat to suit the individual, but a good general basis for it is a liberal supply of milk and eggs with a reduction in the amount of solid food. My own formula for years has been: one meal of solid food a day, and three quarts of milk and six raw eggs to make up for the other two meals. My one meal I direct to be of the most liberal character: beefsteak, roast beef, roast mutton, mutton chops, fresh vegetables, fresh fruit, and an hour to eat it in. The time for this meal should always be specified by the physician, and instruction should be given the patient to masticate his food well and to take all the pleasure possible in the eating of it. A large meal eaten with relish will give a better return than many small meals taken with disrelish. The amount of time, moreover, required for digesting a large meal is practically the same as the amount of time required for digesting a small meal. By limiting the solid food to one meal a day the burden thrown upon the stomach is lessened.

Three quarts of milk and six raw eggs a day amply make up for the two meals which have been cut out and supply a form of easily digested food which gives a large return in energy for the burden which is thrown upon the economy. The large amount of liquid in this diet probably is a benefit also in that it helps to keep the kidneys well washed out, and by leaving a very small amount of residue of undigested food lightens the burden of the intestines. I usually advise my patients to eat fruit and nuts between times, if the one meal and three quarts of milk and six raw eggs do not satisfy them, but I prohibit them from using any other form of solid food until they have regained a normal weight.

Oxygen is just as necessary as food for proper nutrition and it is, therefore, of primary importance for the manufacture of nutrition, to supply the requisite amount of oxygen. Patients should be kept in the open air night and day, and if they cannot be kept out of doors comfortably they should be kept indoors with indoors so well ventilated as to make it practically the same as out of doors. The living room and the sleeping room of the patient, if he sleeps in the house, must constantly have all the windows open, and cross ventilation is absolutely necessary for a proper air supply. Draughts can be entirely disregarded except in so far as it may be necessary to make the patient comfortable. A patient may well sit between open windows if he is properly clothed.

Fresh air is necessary for another reason besides furnishing the proper amount of oxygen for the oxidization of food. Re-breathed air



is a poison and the individual who is struggling against tuberculosis is already overburdened with toxin, and should not have any of his own making re-introduced into him. The atmosphere has the power of cleansing itself in the open, but not when it is restrained within an enclosure. When we exhale in the open air that which we throw out goes beyond our reach and the air which we take in, even though it may have some impurities of various kinds, is at least free from the impurities and poisons given off by our own bodies. The tuberculous subject, therefore, should always breathe air which has not been breathed before and should be so placed night and day as to be able to do this.

As to medication I personally believe that much can be done to aid recovery from tuberculosis by the use of properly selected drugs. In a general way drugs should be used to restore physiological functions of the body when these have been deranged. If the functions of the stomach are incomplete they should be supplemented by artificial digestion. When the eliminatory organs are doing their work improperly, they should be corrected and aided. When the heart is overactive it should be quieted down. No drugs, however, should be given which dry up the secretions and excretions and which interfere with the natural process of recovery instituted by nature itself. Opiates and depressants of all kinds should be avoided. Symptoms should never be treated under any circumstances, but should always be looked upon as an indication of nature's effort to restore the individual to a normal condition. Cough can usually be controlled by will power as much as it needs to be controlled. If the secretions are not free enough and the cough is thereby made excessive, these should be stimulated and not repressed. A good general rule to lay down in medication is to give no drug except for a specific reason in harmony with nature's laws and not in contradiction of them. Where a drug can help elimination, help secretion or help to bring back an organ to its normal function, it may be used advantageously; but no drug should ever be used to quiet the cry of an organ for relief because that relief is not forthcoming, and to shut off the very action which the organ is crying for.

There are no specifics in tuberculosis, but there is at least one drug which is of very great value, on general principles, as an adjuvant, namely, iodine. Iodine has been used in many ways in the treatment of tuberculosis, from time immemorial, and in whatever form it has been used it has been found useful. I have personally for many years used it by inunction in the form of one of the rich compounds which break down readily. I first used iodoform and more recently euophen as less offensive to the patient. I give inunctions with a saturated solution of euophen in olive oil twice a day and I use a similar solution as an inunction for the lymphatic tissue of the pharynx. I am convinced that I have obtained more benefit from the use of this drug in this way than from any other medication I have used. Nutrition improves rapidly under these inunctions and the local signs of the disease disappear. How the

iodine acts I am not prepared to say, but of the results I have not the slightest doubt.

Some of the other drugs which I use quite frequently are nitroglycerine, sulphate of magnesia, strychnia, arsenic, pepsin, pancreatin, hydrochloric acid, phosphoric acid and sometimes digitalis. These drugs I use when they are indicated, to re-establish a physiological function which has been weakened.

Tuberculosis in the early stage can be treated successfully at home in any climate. In cases in which there is no broken down tissue, treatment in the home of the patient is preferable to any other. The treatment brings with it the training for after life. When a patient has recovered away from home, either in a sanatorium or in a changed climate, he often has much difficulty in settling down to the kind of home life which is necessary for maintaining the health which he has recovered.

All cases cannot be treated at home, however. For various reasons some patients cannot be brought under discipline at home. The fault may be in their own physical or mental make-up, or it may be in their environment. Such cases should be treated in a sanatorium in the climate to which the individual has been accustomed. It is better to do this than to change climate, since it eliminates the element of re-climatization from the after life of the individual.

After sanatorium treatment has restored the individual to physical health, home treatment should be instituted to equip him for practical life. Under no circumstances should a patient be thrown upon his own resources after sanatorium life until he has been trained to live properly in his old sphere, or in any new sphere that he may take up.

## THE NATURE OF CANCER AND RECENT ADDITIONS TO THE METHODS OF TREATING INOPERABLE CANCERS.\*

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By FRANK J. LUTZ, M. D., of St. Louis.

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I return to you my sincere thanks for this opportunity to present to the members of the Missouri State Medical Association, and through them to a wider circle, a review of our present knowledge concerning the nature of cancer and, based upon it, the limits of its treatment.

Cancer is world wide in its distribution. Concerning its biology medical knowledge is crude and immature; it occurs upon the surface of the body and in the internal organs; it exists before birth; it saps the full grown body and increases the burdens of ripe old age. Animals and man are afflicted with it. Its presence entails horrible, loathsome suffering. Internal medication fails to cure it; the charlatan only, and the quack and ignoramus instill false hopes of recovery into the heart and mind of the doomed victim in exchange for his gold.

What a field for science! What opportunities for philanthropic endeavor!

Cancer is a disease of the skin, mucous membranes, or of the glands in which tumors are formed. These tumors grow by the proliferation of their tissue elements, at the site of their origin and in other localities to which they may be carried by implantation, or by the blood or lymph currents; there is no limit to their growth; they displace and injure neighboring structures; they have a tendency to disintegrate so far as their older component elements are concerned, thus losing parts of their substance by ulceration, and they destroy the body by producing a cachexia and by auto-infection. This definition, as is apparent, expresses the clinical observation of the disease.

Morphologically a carcinoma is a tumor in which the stroma and the parenchyma are distinctly separated. The parenchyma cells do not form an intercellular substance and do not enter into organic union with the stroma, in contradistinction to the sarcomata, in which an intercellular substance is formed by the parenchyma cells and direct continuity with the stroma is established.

At once the question arises: What causes this riotous and purposeless cell proliferation? This old question has been answered upon many theories. I shall not review any of them, but will simply mention that during the life history of a cancer the behavior of the tumor is such that many very acute and experienced observers believe themselves justified

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\*Oration in Surgery delivered at the Fifty-second Annual Meeting of the Missouri State Medical Association, Jefferson City, May, 1909.



in assuming the parasitic origin of cancers, and have directed their energies toward the discovery of the microorganism; whilst others have asserted that they have found it. Equally careful investigators, more particularly the pathological anatomists, deny the causative relation between the microorganism and the new growth, some admitting the possibility of it, others as strenuously gainsaying even this.

I need not repeat to you that in order to prove clinically the existence of a specific microorganism it must be shown that the microorganism is transmissible from man to man and that the disease produced by it is inoculable.

In spite of the extensive investigations which have been made, this proof has not as yet been furnished. The transference of cancer from one part of man to another part, or from a cancerous person to a healthy one, are not instances of inoculation but of transplantation, and the alleged inoculation of a husband by his wife, or of an operator by the patient, are reasonably explainable upon other grounds than the inoculability of cancer.

But let us view the cancer, or rather the cancer cell, so called, from the general viewpoint of biology.

Nature has no pathology. Since the days of Bichat and Laennec and the epoch-making work of Virchow, it is well established that pathological processes are subject to the same laws which govern physiological processes. The laws of biology furnish the explanation for the phenomena observed in cancer. Our knowledge concerning the minute changes which are observable in cancer has arrived at a high degree of accuracy—in fact it would appear that the very limit of anatomic information concerning this disease has been reached.

What then are the peculiar characteristics of a cancerous new growth; and how can we explain them by the general laws of biology?

It is well proven that want of nutrition and over-feeding, as well as irritations of all kinds, produce cell multiplication.

By way of illustration: Gunshot injuries to nerves are followed at first by increased growth of the epidermoid structures, the hairs and the nails, soon to be followed by their atrophy. Uterine cancers take on a more rapid growth during pregnancy. An erosion subjected to constant irritation may develop into a cancer.

The low vitality of cancer cells is as pronounced as their exuberant growth. Observe the luxuriating callus in certain fractures—increased cell formation continues as long as the irritation lasts, and only after the irritation ceases, and with it the increased nutrition, does absorption take place—otherwise a cellular new growth is formed. Regeneration is but an exaggerated normal process, and physiologically organs and tissues are constantly renewed, each cell being supplanted by one newly formed.

Temperature, water, osmotic conditions and light have their potent influence in cell development, both in the animal and vegetable kingdoms.

Rapid frequent cell multiplication follows increased nutrition. In cancer the supply of nourishment is constantly increased and hence follows rapid cell production. The less developed a cell, the more speedily and the more frequently does it multiply—and a cell which produces daughter cells, whilst in a stage of immature development, transmits to its offspring the tendency to propagate itself during a low stage of development and to produce cells of smaller size. Rapid development is the characteristic of small lowly organized beings—applied to cancer it is well established that as the tumor grows the cells become smaller and develop more rapidly, presenting greater malignancy.

Has our therapeutic success kept pace with our knowledge concerning the minute structure and our biologic knowledge of cancers? Unfortunately the word "cure" is still part of the vocabulary of the physician and surgeon as well as of the laity. The average medical man thinks of a cure in connection with an external cancer, and takes only that wider view of the object for which treatment is instituted in this disease when the new growth affects an internal organ. Too much is expected because too much has been promised in connection with the efforts of medical men in the presence of cancer. The layman who is told that his disease is cancer at once asks the question: Can it be cured? The physician, unfamiliar with the clinical course of cancer, or lacking the moral courage to confess his impotency, in many instances answers the query in the affirmative, only to be doomed to disappointment, together with his patient.

Now what may be expected as the result of treatment? In malaria, an infection by the plasmodium, we have a specific in quinine. By administering this drug and by removing the patient to places not infected with mosquitoes or by destroying the carriers of the infection, we cure our patient. By removing the ingesta, which caused the ptomain infection, the patient is cured. A pneumonia, running its natural course, terminates in the recovery of the patient and, in so far as the physician has guided him through the series of disturbances grouped under the head pneumonia, the physician has cured him. Not so with cancer. In speaking of the cure for cancer as the result of anything which the physician might do, we must keep in mind two things: First, that we are dealing with a local disease, and secondly, that this local condition undergoes such disintegrating changes as to affect the general system. The answer, therefore, as to whether a cancer, in a given instance, is a curable disease, will depend upon the local condition, upon the nature of the tumor, the extent to which it has involved the neighboring structures including the glands, and the general condition of the patient, which is represented by his toxic condition.

The recognition of the nature of the local disease, or, in other words, a correct diagnosis, calls for ability acquired only by careful study and by experience. The diagnosis of a cancer upon the surface, when first it begins to develop, is by no means an easy matter. And when located in

organs not directly approachable, it calls for the exercise of the highest diagnostic skill, and presents problems the most difficult of solution. To illustrate:

A fissure in the mucous membrane of the lower lip, associated with the oft-produced irritation of the stem of a pipe, may be a cancer, but it may also be luetic. An incipient carcinoma of the stomach calls for investigations prolonged, painstaking and extensive. The mammary carcinoma, represented by a hard, nodular tumor, with a retracted nipple or dimpled skin, and with axillary glands hardened and enlarged, is correctly interpreted by one of very limited experience. Malignant new growths in the central nervous system, the brain or the spinal cord, give rise to symptoms by no means readily deciphered.

I refrain from reciting the local characteristics of cancer; a knowledge of them is the common property of medical men. But an extensive experience leads me to believe that the question of treatment is one upon which most practitioners are not agreed. A rapidly growing tumor infiltrating surrounding structures, hard, painless, fixed and associated with the peculiar cachexia indicative of the absorption of toxins into the general circulation, readily suggests carcinoma to one of experience.

Unfortunately, in spite of the great frequency with which cancer occurs, medical men have not given that attention and acquired that skill in its recognition which is possessed by them in regard to many other equally common affections. The knowledge of medical men, and of the public, concerning tuberculosis has been wonderfully increased during the last few years by an agitation which threatens to overshadow every other consideration in matters medical and sanitary. A similar agitation, less noisy and more conservative, is going on in regard to cancerous diseases. In the education necessary for the recognition and appreciation of this condition the medical profession must begin by first educating itself and then the public.

It is not the part of wisdom to rail against the quacks and pretenders whose diagnostic skill may surpass that of the physician, who justly criticizes the methods and promises of the pretender, but does not possess equal diagnostic skill and is on no higher plane concerning the limitations of therapeutic endeavor in cancer than is the quack.

Since history has been written cancer has been known and described as a destructive, incurable disease, and physicians and surgeons have at all times sought means to combat it. But not until the 18th century do we learn of an earnest warfare against this terrible affliction, as it found expression in scientific observation and investigation, as well as in the care of the unfortunates.

As early as 1701 a ward was set aside exclusively for cancer patients in the Middlesex Hospital in London, and in 1801 a society for investigating the nature and cure of cancer was formed. A few years later Mrs. Adithea Mary Stafford gave 36,000 pounds for the endowment of 26 beds for cancer patients. In 1851 the Brompton Cancer Hospital



was founded. In France a ward in the Salpetriere was assigned to these afflicted ones and many prizes were established in order to encourage the study of this disease.

I refer to these historic facts merely to show that general attention was directed to the disease by medical men as well as by philanthropists long before the time when the biologic study of it was begun.

Positive knowledge of cancer was of slow growth and exact study of tumors began only with the development of pathological anatomy.

The great Berlin physiologist, Johannes Mueller, was the first to show, by microscopic examination, that tumors are composed of cell elements which correspond to those of the human organism—and Virchow elaborated in detail the composition of the varieties of new growths.

We must, therefore, first learn to recognize the disease and then we must be familiar with its clinical course, as regards the result of its continuous growth upon the economy and eventually upon the life of the patient; and also must we know what have been the results of the various methods of treatment by which the disease has been either removed by surgical operation or attacked by other means.

Extensive knowledge gained by clinical experience furnishes the best foundation for honest dealing with the patient. Ignorance only makes prognostications in conformity with the wishes of the patient. We know, and this from the universal experience of observers, that by far the largest number of cancers, whether treated by one method or by another, whether removed by the knife or destroyed by escharotics, return. We also know that the clinical course of cancers is erratic; that no definite law governs their course and termination in all instances. We know that there is a natural cure for cancer; we know that nature endeavors, and successfully, in many instances, to counteract locally the disease by destroying the tumor soon after it is formed.

In our own day the scientific and practical study of cancer has attracted not only the attention of individuals but societies have been formed in all countries for the study of its medical and sociological bearings. The study of cancer has received an unusual impetus by endowments on the part of philanthropists, of hospitals and laboratories. Much work is being done, especially in Europe, by societies devoted exclusively to its study, aided and assisted by the governments of the respective countries. The fruits of these endeavors were thought sufficiently great to induce the second meeting of the International Surgical Society, which held its session in September, 1908, at Brussels, to devote the larger portion of its session to the consideration of cancer. It was my privilege to attend this meeting as the representative of the St. Louis Skin and Cancer Hospital, and submit our modest contribution to its truly wonderful exhibit. It will be no digression from our subject to say that the contributions to the cancer exhibit of this Congress afforded an opportunity rarely enjoyed. The illustrations furnished by the pathologists, the exhibitions of patients upon whom various operative

procedures had been practiced, the means and methods adopted in the laboratories and the results of laboratory work in regard to the biologic characters of cancer, as well as the exhibition of instruments of precision and of plans of hospitals devoted to the treatment of cancer patients, have never been equalled.

So far as I know, only the state of New York is governed by a spirit so broad and so elevated as to take into consideration cancer as an important factor in the health-disturbing causes among its citizens. It makes annually a dignified appropriation for the purpose of studying this disease in the hospitals and laboratories at Buffalo, and from that center have emanated many suggestions of value about this disease.

It should be a matter of congratulation to the citizens of this State that among its charitable institutions is one devoted exclusively to the study and treatment of cancer. I refer with pardonable pride to the George D. Barnard Free Skin and Cancer Hospital of St. Louis, recently endowed by this distinguished philanthropist. Since first its doors were opened to these unfortunates on the 1st of June, 1905, until May 1st, 1909, there have been treated in it 881 cases of cancer of all varieties.

It has for its object, first, the amelioration of the condition of the unfortunates afflicted with this dreadful disease and to whom, when they have reached the incurable stage, other institutions, whether conducted as charitable or whether a return is expected for the care of the patient, furnish no welcome shelter. In the next place it is intended that from this institution shall emanate such advice to the medical profession as is the result of the experience there gained, and therefore ample provision has been made in it for treatment by all suggested reasonable methods.

In addition, one of the great objects for which it was founded would not be accomplished were the medical profession deprived of the opportunity to study and observe for themselves in its wards the cancer disease, and before long those who desire to take advantage of it will be afforded a chance to avail themselves of the material for their personal instruction.

But besides the medical man the laity must be instructed concerning the nature and the behavior of cancer, as well as the possibility of curing it when eradicated early or of ameliorating the conditions when the disease is so situated as not to be directly approachable by surgical means, or when its ravages are so great as to preclude the hope for its extirpation.

We have a practical working theory when we consider cancer an infectious and contagious disease, and its behavior practically justifies this, and therefore if knowledge concerning it is disseminated among the people, based upon this theory, the vultures who now prey upon the fears and sufferings of cancer victims will find their harvest materially reduced, although I am not so optimistic as to believe the time will ever come when human distress and human suffering will not lend a willing ear to promises of relief.

The direct attack of a malignant new growth surgically implies its thorough removal, the organ or immediate structure in which it originates or of the surrounding tissues to which it has extended, as well as the planes of extension, the fascia and fat structures together with the lymphatic glands which drain the areas involved.

When cancer occurs in organs or tissues from which it is not removable, or when its removal would necessitate procedures of themselves life-endangering, the institution of palliative procedures with a view to alleviating suffering and prolonging life will tax the technical skill and physiologic knowledge of the surgeon. The surgery of cancer, although operative possibilities seem to have reached their limit, has by no means attained results in keeping with the hopes of the operator. Entire organs have been removed, parts of a lung, sections of the brain, some of the organs of digestion have been extirpated in toto or parts of them have been removed, and the organs of generation have been excised, and yet with it all, if value is to be attached to statistical numbers, about 60 per cent. of returns confront us.

Fortunately, however, the time limit during which these returns occur has been greatly lengthened. Not so many years ago surgeons pointed with pride and much satisfaction to cases in which the patient, after an operation, had remained free from the return of the disease for three years, and considered such a patient cured. Already this period has been extended to five years and it is confidently to be hoped that as the disease becomes earlier recognized and the laity and physicians become convinced of the advantages which accrue from early removal, patients early operated upon will enjoy immunity from this disease for a still greater period.

I wish to emphasize in connection with the surgery of cancer, the view which I fear is not taken by many who consider the display of operative pyrotechnics the acme of surgical achievement. Operative surgery should be only a part of the treatment of cancer, although the major one; but we should never lose sight of the fact that in cancer there do occur disturbances of metabolism, either preceding the formation of the new growth or consequent upon it, which call for the exercise of much discrimination and for the knowledge of which we are just beginning to lay the foundation. Our increased knowledge of physiology and dietetics, including the administration of such drugs as arsenic and iron, gives promise to aid a cancer patient, next to the mechanical means employed for the removal of the new growth.

I said a few minutes ago that the clinical course of cancer is erratic—not governed by fixed laws. In some forms the growth of the tumor ceases without known cause and without intervention; on the other hand, experience teaches that in no inconsiderable number of cases, especially when the new growth is of a diffuse character, subacute in its behavior, when it occurs in young persons, the removal of the tumor by



the knife hastens its extension and return and the destruction of the individual.

Again, there occur in older patients slowly growing tumors which do not greatly disturb their comfort and which should be left severely alone, unless excessive pain, ulceration or hemorrhage demand interference. Between these extremes must be grouped that large class of malignant tumors in which early, thorough removal confers great benefits and promises complete cure.

For inoperable, neglected cases any method which gives promise of even ameliorating the condition should be welcomed.

The observation that the Roentgen ray affects the skin of those exposed, which was made soon after the discovery of the  $x$ -ray, led at once to its therapeutic use in cancer. Clinical experience has not sustained the enthusiastic claims of its advocates, but has restricted its use to superficial cases in which it causes destruction and exfoliation of the cutaneous cancer in the same manner as degeneration is brought about by other causes. And since this destructive process is slow and superficial in its action it has no advantage in many cases in which the disease can be permanently removed by a slight surgical procedure. Furthermore, it has no special affinity for the cancer cells and therefore when used so strongly as to destroy cancer tumors more deeply situated it also destroys the surrounding tissues.

It has been my practice, however, to give all cases, in which it has been practical, such benefits as may come from a post-operative course of  $x$ -ray exposure.

Nor should we forget that there is some truth in what our surgical predecessors observed at the bedside, when they taught that a cancer which had been removed by the knife and in which the removal was followed by suppuration, did not recur as soon as when immediate union of the operation wound was secured. Not that I would be understood as advocating wound infection as a method of after treatment—the futility in carcinoma of infection artificially induced by the bacillus erysipelas warns against this—but a wound surface remaining after thorough excision, allowed to heal by granulation and exposed to the Roentgen rays, gives greater promise of having destroyed in it all the cancer tissue than one in which the wound is hermetically sealed or only slightly drained.

In inoperable uterine cancers with hemorrhage, excessive discharges and foul odors, the methodical application of acetone, first suggested by Dr. George Gellhorn, one of the attending gynecologists of the St. Louis Skin and Cancer Hospital, has been found not only to destroy the bad odors, but also to diminish the discharges and to limit the hemorrhage.

Coley's serum has gained for itself a permanent place in our treatment of inoperable sarcomata more particularly, and is sufficiently well known to workers in this field.

Another method of treating inoperable cancer is that introduced by Keating Hart—the treatment by fulguration. The cancerous tissue is

removed by the sharp spoon, the knife and scissors, and upon the bleeding area a strong spark of electricity is made to play under profound chloroform anæsthesia, upon superficial and upon deep-seated cancers. The metal electrode is fed by a high frequency, high tension electric current so that the spark may reach 6 to 10 c.m. in length. This spark of lightning, which is cooled by passing it through a stream of carbonic acid, destroys the cancer cells, and also the intercellular substance, and produces upon the surface of the cancer, or in it if conducted into the tumor through penetrating needles, an eschar under which rapid granulation formation and early epidermization takes place. The involved glands are enucleated or incised and then fulgurated. One of the systemic effects during fulguration consists in the production of a kind of hypnotic sleep which permits the discontinuation of the chloroform anæsthesia.

In addition, hemorrhage ceases at once. The wound surface is covered with sterile gauze of sufficient thickness to absorb the large quantity of lymph which is poured out as soon as the local reaction takes place. This lymphorrhea continues for many days; at first the fluid is reddish in color, but soon becomes yellow and serous, and gradually diminishes in quantity. Morphologically, it contains polynuclear cells in abundance. In the presence of this abundant serous flow, no plastic closure of the wound should be attempted, but free drainage established and continued to prevent systemic infection.

The liability of explosion precludes the use of ether, as a rule.

The exact value of this method cannot, as yet, be stated. A wider experience alone will decide what place will be assigned to it.

In his relationship to others, the cancer patient must be considered as suffering from an infectious and contagious disease. If this working theory accomplishes nothing else, it will insure that surgical and social cleanliness which will make the sufferer better able to bear his affliction and which will make for the protection of his fellow-man.

There is no specific cure for cancer.

## A CASE OF MELENA—RECOVERY.

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By WILLIAM THOMAS DORAN, M. D., of New York City.

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I am indebted to Dr. Ernest Walker, who left this case in my charge during his absence from the city.

Baby R., born 7:30 A. M., August 15th, 1908. Weighed five pounds and 15 ounces. A normal child in every way. Labor normal. The mother, when a child, had a tendency to bleed. A maternal aunt had placenta praevia.

Nothing of any importance until 8:45 P. M. of the following day, when the baby vomited a small quantity of dark blood, and seemed uneasy, and cried a good deal.

At 1 A. M. the following morning, the baby's stool was bloody. A saline enema was given, the water returning bloody; temperature 102.8. Increased vomiting; it seemed as if the mucous membrane of the mouth and nose oozed blood, of a bright red color. Vomiting continued at short intervals; clotted blood from the stomach. Stomach was washed; water returned containing clots. The following morning at 9:30, temperature down to 99.8. At noon, August 17th, consultation with Dr. C. G. Kerley, and diagnosis of melena verified; temperature, 99.8. Stomach again washed, water returning bloody. Through stomach tube administered gr. 2, lact. calc. in dr. 2 of hot water.

The child seemed to urinate a sufficient quantity. At the same time lact. calc. gr. 3, and peptonized cow's milk, were given per rectum. This was not retained and returned mixed with clotted blood. Drops ten adrenalin chloride, were given per mouth. This treatment, washing of stomach and feeding per rectum, was kept up until the afternoon of the 18th, when, with consultation, it was thought best to transfuse, child growing weaker. Transfusion unsuccessful.

On August 19th, temperature 104.4; child very weak. Stools still slightly stained with blood; vomited less blood from stomach. Baby seemed at that time inclined to keep head in backward position more or less fixed. Lact. calc. was kept up, per mouth 2 gr., per rectum 3 gr., as heretofore. Mother's milk was given to the child, peptonized, per rectum.

As far as I have been able to find this is the first case fed in this way. Temperature on the decline until 8:30 of the 19th, when it dropped to 99.8. Stools still contain small quantities of blood; very little blood vomited. Drops two of adrenalin continued by mouth.

On the 20th the child was fed mother's milk by spoon, plus nutritive enemas of mother's milk peptonized. Child seemed to retain mother's milk per rectum much better than cow's milk. Gradual decline of blood



per vomit and per rectum. Mother's milk was increased to two dr. per mouth.

On the afternoon of the 20th, child was put to breast. Seemed to get milk at times; made quite an effort at nursing. Bowel was always washed before enema. On the afternoon of the 20th, washing returned clear. Adrenalin, drops 2, with 10 drops of saline, continued every four hours. Lact. calc. per mouth and rectum continued. Child not as restless as heretofore.

On the 21st the child became restless, crying; seemed thirsty. Temperature rose to 104.8. Very little trace of blood in stools. When put to the breast would not nurse, and had to go back to feeding with spoon. On the afternoon of the 21st, temperature went down again, until in the evening it was 99.6. The child seemed unable to nurse and was fed by mother's milk per mouth and rectum.

Weight of child on the 21st, 4 pounds and 9 ounces. Still continued adrenalin as well as lact. calc. per mouth and rectum. All this time there were no signs of any infection in the navel.

On the 22d child weighed 4 pounds and 11 ounces, able to nurse a little. No bleeding after the sixth day.

On the 23d child weighed four pounds and 15 ounces.

On the 26th of August cord came off. No discharge. Child weighed five pounds, one ounce. On the following day, five pounds and two ounces. Next day five pounds and five ounces. Adrenalin and salines stopped on the 26th. Lact. calc. diminished to grains 2 given by mouth only.

After August 28th the child did not have any nutritive enemata, but took the breast. Stools became normal color.

Continued lact. calc. gr. 2, by mouth, twice daily until January, 1909.

Child now shows some symptoms of rachitis; April 12th, 1909, weights 16½ pounds.

356 Lexington Avenue.

# MEDICAL AND SURGICAL PROGRESS.

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## AIDS TO THE DIAGNOSIS OF TUBERCULOSIS, CHIEFLY BY MEANS OF FINDING THE TUBERCLE BACILLUS.

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A REVIEW OF RECENT LITERATURE.

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By ALBERT E. TAUSSIG, M. D.

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1. EARLY DIAGNOSIS OF PULMONARY TUBERCULOSIS BY MEANS OF THE EXAMINATION OF THE GASTRIC CONTENTS.—Hausmann (*Deut. Arch. f. klin. Med.*, Vol. 94, Nos. 5-6).
2. A NEW METHOD OF FINDING THE TUBERCLE BACILLUS.—Lange and Nitsche (*Deutsch. med. Wochenschr.*, 1909, No. 10).
3. THE USE OF ANTIFORMIN IN THE DETECTION OF TUBERCLE BACILLI.—Seemann (*Berl. klin. Wochenschr.*, 1909, No. 14).
4. A NEW CONTRAST STAIN FOR INTRACELLULAR TUBERCLE BACILLI IN THE SPUTUM.—Assmann (*Muench. med. Wochenschr.*, 1909, No. 13).
5. THE SHAPE OF THE LEUCOCYTES IN THE URINE OF URINARY TUBERCULOSIS.—Goldberg (*Deutsch. med. Wochenschr.*, 1909, No. 13).
6. THE PRESENCE OF TUBERCLE BACILLI IN THE CIRCULATING BLOOD IN TUBERCULOSIS.—Rosenberger (*Am. J. Med. Sc.*, 1909).
7. AN ERROR SOMETIMES MADE IN EXAMINATION OF SPUTUM FOR TUBERCLE BACILLI.—Buhlig (*Quar. Bull. Northw. Univ. Med. Sch.*, quoted in *Jl. A. M. A.*, 1909, No. 24, p. 2055).

Current medical literature is full of contributions dealing with the early diagnosis of pulmonary tuberculosis. One of the most interesting is an observation recently made by Hausmann. Being a gastroenterologic specialist he had frequent occasion to see patients complaining of gastric disturbance in whom no definite gastric lesion could be found, but in whom he thought himself able to demonstrate a beginning apical tuberculosis. Often there was apparently no cough and therefore no sputum, so that the tubercle bacilli could not be demonstrated. He habitually referred such patients to pulmonary specialists and frequently had the humiliation of having them sent back to him with a report denying the presence of tuberculosis. It finally occurred to him that in spite of the absence of cough, small particles of sputum might be cast up into the pharynx and swallowed, especially during the night. The fasting stomach contents of such patients might then contain tubercle bacilli. The first case, in which he acted upon this hypothesis, gave him a positive result. The fasting stomach contents

contained a small sputum-like lump which, when stained, was found to be full of tubercle bacilli. Within two months he was able to demonstrate tubercle bacilli in the fasting stomach contents of five other patients, in whom, on account of the absence of cough and sputum, the diagnosis had seemed doubtful. Of course this procedure does not rank with the finer diagnostic methods that enable us to diagnose a closed pulmonary tuberculosis, but in many cases it will doubtless prove to be of the greatest service, especially where there is a difference of opinion among consultants. Our own experience with this method has been satisfactory. It is usually more satisfactory to wash out the stomach with a little water rather than try to express the contents undiluted. The little cheesy or purulent fragments may then be seen floating in the wash-water and can readily be picked out.

Lange and Nitsche suggest a ready method of finding tubercle bacilli in sputa in which the bacilli are very scanty. They found that when a watery suspension of tubercle bacilli is shaken up with a hydrocarbon, the bacilli tend to attach themselves to the tiny droplets of hydrocarbon, and are carried to the surface or to the bottom according to its specific gravity. As applied to the sputum their method is as follows: 5 c.c. of sputum are shaken up with 50 c.c. of normal KOH (official liquor potassæ will serve) and allowed to stand until completely homogeneous, shaking occasionally. To the mixture, 50 c.c. of water are then added and 2 c.c. of some hydrocarbon, preferably ligroin, shaking vigorously until an emulsion results. The entire material is then gently warmed (60-65° in a water-bath) whereupon the ligroin collects on the surface of the mixture, in a clear layer. A considerable number of loop-fuls of fluid are taken from the area of contact between the ligroin and the water. These are placed on the same spot on a slide or cover-glass, allowed to evaporate and stained by the usual methods. Any hydrocarbon may be used, but the heavy ones that settle to the bottom are not readily accessible to the platinum loop, while the lightest ones, such as petroleum, ether, benzin and the like, tend to run all over the cover-glass making it difficult to obtain a thick spread. Of all the hydrocarbons tested ligroin seemed the most advantageous. Diphtheria and pseudo-diphtheria bacilli, as well as the other acid-fast bacteria, behave similarly, but none of these interfere with the diagnostic value of the method.

About a year ago, Uhlenhuth found that antiformin has the property of dissolving rapidly all kinds of organic material without, however, apparently having any effect upon tubercle bacilli. Antiformin is a mixture of Labarraque's Solution (liquor sodæ chloratæ) with sodium hydate, the proportion not being stated. It has a powerful oxidizing action. Seemann uses it in a 15 per cent. solution. The sputum is mixed with 15-20 volumes of the solution and allowed to stand in a conical glass. Disintegration begins at once and in a few minutes cheesy particles may often be picked out that can readily be crushed and stained or even used for purposes of cultures or animal inoculation. It is usually best to wait until disintegration is complete, which requires from 10 to 45 minutes. The mixture then consists of a turbid fluid with a more or less plentiful sediment consisting of detritus and tubercle bacilli. A bit of the sediment is fished out and stained with carbolfuchsin. The method may be used for sputum, pus, urinary sediment, exudates, stool, or even entire organs, such as tuberculous glands, which, if first cut into small pieces, are readily disintegrated by the fluid.

The work of Wright has attracted attention to the possible importance of intracellular tubercle bacilli in the sputum. If it is true,



that the ability of the leucocytes in the body to devour tubercle bacilli, is a measure of the ability of the organism to cope with the infection, then the prevalence in the sputum of tubercle bacilli lying within leucocytes indicates a favorable prognosis. This hypothesis is as yet not definitely established, although hitherto such intracellular tubercle bacilli have been observed chiefly in the sputum of very chronic cases, or of patients who had been long treated by means of tuberculin injection. The ordinary methods of staining tuberculous sputum do not permit the detection of intracellular bacilli since the latter cannot be distinguished from bacilli lying above or below pus cells. Assmann has found the following stain useful for this purpose:

1. Fix and stain a very thin spread of sputum on a slide in the usual manner with hot carbol-fuchsin for one minute. Decolorize first in 5 per cent. sulphuric acid and then in absolute alcohol until the spread is macroscopically colorless.

2. Wash in running water at least half a minute and dry with filter paper.

3. Place the dry slide in a clean Petri dish, cover with 40 drops of Jenner's stain, so that the fluid does not run over the edge of the slide and stain 5 minutes.

4. Pour into the dish 20 c.c. distilled water, to which 5 drops of a 0.1 per cent. solution of potassium carbonate has been added. Agitate the dish gently until a homogeneous violet solution results. Allow this to stand for 3 minutes.

5. Remove the slide, rinse gently with distilled water and dry carefully with filter paper.

The tubercle bacilli are stained a brilliant red and, if intracellular, lie within the pale rose of the protoplasm surrounded by a narrow colorless areola. The latter enables one definitely to distinguish between true intracellular bacteria and those that merely lie in contact with the pus cells.

Some years ago Colombino made an interesting observation. He found that in non-tuberculous infections of the urinary passages, the leucocytes in the urine are round, sharply contoured and readily stained. In urinary tuberculosis, however, they are elongated, polyhedral or jagged; they have excrescences, so that their cell-membrane seems burst. In the stained preparation, the leucocytes have an irregular outline, the protoplasm contains vacuoles and the nuclei stain poorly. The changes are found not only in the polynuclear leucocytes, but also in the large mononuclear cells. He considers them due to the action, upon the cells, of the tuberculous toxin and believes their occurrence pathognomonic of urino-genital tuberculosis. In view of the impossibility of demonstrating the presence, in the urine, of tubercle bacilli without animal inoculation, this observation, if confirmed, might prove of great diagnostic importance. Goldberg has recently studied a large number of cases of pyuria from this point of view. He found these changes in the leucocytes uniformly present in all cases of urinary tuberculosis, and absent in all cases of calculus. In acute and chronic gonorrhœa, however, the same phenomenon was observed so that these degenerated pus cells can be utilized diagnostically only if gonorrhœa can be excluded.

The most important contribution in regard to the detection of tubercle bacilli is apparently that of Rosenberger. He was able to demonstrate the presence of tubercle bacilli in the circulating blood of every case of tuberculosis examined, whether acute or chronic, incipient or advanced. The matter was discussed in some detail in a previous issue of this

JOURNAL (June, p. 429). The method is simplicity itself. Five cubic centimeters of blood are mixed with an equal amount of 2 per cent. sodium citrate and 1 per cent. sodium chloride solution. After standing over night in the ice-box a little of the sediment is dried on a slide, laked with distilled water and stained in the usual manner. The bacilli, it is said, can usually be found without very great difficulty. The writer of this abstract has not been very successful with this method, having been unable to find the bacilli in a considerable number of clear cases of tuberculosis. Whether this is due to lack of patience, faulty technique or actual absence of the bacilli, remains to be determined.

Artefacts in the sputum are not usually of such a nature as to lead to serious diagnostic error. Buhlig, however, points out that in the attempt to hurry a preparation of sputum for microscopic examination, the smear is often heated before it is quite dry, producing in this way artefacts that are sometimes with difficulty differentiated from tubercle bacilli.

## BLOOD PRESSURE.

## A REVIEW OF RECENT LITERATURE.

By JESSE S. MYER, M. D.

1. BLOOD PRESSURE IN MAN, ITS MEASURE AND REGULATION.—Brunton (*Lancet*, October 17, 1908).
2. STUDIES OF THE RELATIONSHIP BETWEEN RESPIRATION AND BLOOD PRESSURE.—Lewis (*Journal of Physiology*, 1908, page 213).
3. CONCERNING THE FUNCTIONAL SOLIDARITY OF THE TWO SIDES OF THE HEART.—Kraus and Nicolai (*Deutsche med. Woch.*, January, 1908, No. 1).
4. CAUSE OF SUDDEN FALL IN BLOOD PRESSURE WHILE EXPLORING THE COMMON BILE DUCT.—Ransahoff (*Annals of Surgery*, 1908, Vol. 48).
5. INFLUENCE OF CO<sub>2</sub> BATHS UPON THE BLOOD PRESSURE.—Hürter (*Zeitschr. für phys. und diätet. Therap.*, 1908, Bd. 12).
6. THE BLOOD PRESSURE.—Elsner (*N. Y. State Journal of Medicine*, July, 1907).
7. METHODS FOR DETERMINING THE BLOOD PRESSURE AND THEIR PRACTICAL SIGNIFICANCE—F. Kraus (*Deutsche med. Woch.*, February 11, 1909).

Sir Lauder Brunton gives a very complete review of the subject of the development of blood pressure technic, and the various instruments which have been used in the work. The von Basch sphygmomanometer, Potain's sphygmomanometer, Gaertner's tonometer and the Riva Rocci, Oliver and Martius sphygmomanometers, together with the author's own instrument, which includes an aneroid for measuring the blood pressure instead of the usual mercury manometer, are all pictured and fully described. In normal individuals, the author has found the maximum blood pressure to be as follows:

Between the ages of 8 to 14, 90 mm. Hg.

From 15 to 21, 100 to 120 mm.

From 21 to 65, 120-125 to 150 mm.

In women the average is from ten to fifteen mm. lower, and in strong, athletic men from 10 to 15 mm. higher. He calls attention to the probability of an abnormally low blood pressure being premonitory of phthisis, and suggests further observations on this point. After influenza the fall in blood pressure is sometimes very marked. He has found a powder consisting of potass. nitrate, gr. 15; potass. bicarb., gr. 10; sodium nitrite, gr.  $\frac{1}{2}$ -2, given in a glass of water, mornings, as very efficient in many cases of hypertension.

Several very interesting laboratory experiments with practical significance have recently appeared, among these a paper by Lewis. This author tried to determine on cats the effect of changes in the intrapericardial pressure on the aortic pressure, and comes to the following



conclusions: A rise of intra-pericardial pressure of one mm. Hg. gives a fall of blood pressure in the carotid of eight to nine mm. and this effect is almost immediate, occurring within one to three heart beats. Cohnheim and Frank obtained similar results, although they employed greater variations in the intra-pericardial pressure. The author finds that when relatively large positive pressure is used, the ratio of 1 to 8 tends to increase; when relatively large negative pressure is used, this ratio tends to decrease. The mechanical effect upon the blood pressure of breathing seems to act through pressure on the pericardium, and not through any change of resistance in the blood vessels of the lungs. It has never been demonstrated that change in the diameter of the lung vessels, resulting from alteration in the intra-pleural pressure, has any appreciable influence in the production of the inspiratory rise of blood pressure. In man, a deep intercostal inspiration, not prolonged, yields a pure fall in blood pressure, a deep diaphragmatic inspiration, a rise. It would seem from these observations of Lewis that the clinician in cases of pericarditis and also of hydrothorax from whatever cause, would do well to pay particular attention to the blood pressure and its variations. A continued low blood pressure might be of a decided aid in diagnosing pericardial effusion, though this will have to stand the test of clinical observation. Another series of observations upon animals, though not bearing directly upon the subject of blood pressure, are those recorded by Kraus and Nicolai. By means of the electro-cardiogram the authors were able, in experiments on dogs, to demonstrate that the two sides of the heart may act independently of each other. By stimulating separately the right and left ventricles, typical and distinct electro-cardiogram curves were seen for each. They conclude that in clinical cases which are analogous—*i. e.*, in ventricular extrasystole, it is possible by the electro-cardiogram to distinguish between the right and left sided origin of the phenomenon.

Ransahoff, in experiments upon rabbits, attempted to find the cause of the shock so often attendant upon operations on the common duct. The animals were etherized through a tracheal canula, and the blood pressure determined directly from a canula introduced into the carotid. Opening the peritoneal cavity resulted in a slight fall of pressure with quick return to normal; traction on the gall-bladder produced no effect on the pressure; traction on the pylorus, and thus on the gastro-hepatic omentum, in a pedad direction, produced no effect, but in the ventrad produced a slight fall, seemingly due to a slight kink in the portal vein; vigorous sponging in this region caused a slight gradual fall. The pylorus was isolated and the tip of the finger introduced without pressure on the vena cava, into the foramen of Winslow. In each case at the moment of introduction a sudden drop in the blood pressure of from 20 to 40 mm. occurred with rapid pulse, but when the finger was removed the pressure immediately rose to normal again. The portal vein was now isolated and a temporary clip applied, whereupon the blood pressure immediately dropped 50 mm. and gradually rose to normal again when the clip was removed. Control experiments on the human during operation showed a fall in pressure from 105 mm. to 70 mm. on introducing the finger into the foramen of Winslow. The author concludes that the fall in blood pressure is due to compression of the portal vein with consequent impediment to the blood flow, and suggests that digital exploration of the common duct should be intermittent in order to avoid shock.

Hürter finds that CO<sub>2</sub> baths between the temperature of twenty-nine and 32 C. do not lower a pathological hypertension. Above 32 C. there is a marked fall, which, however, tends to rise even above the original tension after the bath. A lasting effect in regard to tension is not to be had from these baths.

Elsner believes it is as important to take a blood pressure as to examine the urine. Of 600 patients, nine (1.5 per cent.) had chronic hypertension. It is especially important to know the blood pressure in persons between the ages of 35 and 50, in order to guard against overwork. Hypertension and arterio-sclerosis are by no means synonymous terms and arterio-sclerosis is not necessarily a high tension disease. In angina pectoris the tension is often decidedly low, both during and between attacks. In chronic hypertension, hypertrophy of the heart and chronic interstitial nephritis are to be looked for in the course of what should be a thorough physical examination. The hypertension is often the first symptom of beginning interstitial nephritis, and is present before there is albumin in the urine. Thus chronic hypertension calls for repeated urinalyses. "The earliest rise of blood pressure in a syphilitic should arouse suspicion and demands active interference." In the majority of cases of Graeve's disease there has been a normal pressure or moderate hypertension. "We have had a number of opportunities to establish the fact that during the early hours of intestinal perforation in typhoid, blood pressure promptly rises."

Kraus, in an article in which he considers various methods for the determination of the blood pressure and their practical value, says the blood pressure is dependent upon, first, the elasticity of the vessel wall, and, secondly, the volume of the blood which the vessel contains at any given time. The first factor is again dependent upon the condition and tone of the vessel wall, and the second factor depends, first, upon the capillary resistance and the consistency, etc., of the blood, and, second, upon the pulse rate, the volume of blood thrown out at each systole and the total amount of blood of the body. The usual ordinary pressure denotes a certain balance between these various factors. Kraus discards the terms systolic and diastolic pressure as they are at present used, and does not believe that the diastolic pressure, as it is measured with the pneumatic cuff and manometer, is a correct measure of the true physiological diastolic pressure. He also uses the term maximum pressure, "Maximaldruck," instead of systolic pressure. The question as to whether the pressure in the brachial artery is also the pressure of the aorta does not appeal to the author as one of urgent practical importance, since there is certainly some rather close relation between the two which is sufficient for clinical purposes. The author, in a previous communication, called attention to the fact that a determination of the blood pressure alone, without a knowledge of the amount of blood thrown out by the ventricle at each systole ("Herzschlagvolumen") can give us no idea as to the sufficiency of the general circulation. Nor is it proper to assume that a low blood pressure indicates a large ventricular systolic output and vice versa. Recently, Plesch has developed a method for determining this very important factor in an indirect way by determining the amount of CO<sub>2</sub> the individual takes in at a given time, the amount of CO<sub>2</sub> in the circulating venous blood and the amount of CO<sub>2</sub> in the circulating arterial blood. The technic is somewhat complicated, but it is to be hoped that by this method we shall gain an insight into many clinical phenomena as yet little understood.

# APHASIA.

## A REVIEW OF RECENT LITERATURE.

By SIDNEY I. SCHWAB, M. D.

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2. L'APHASIE DE BROCA.—Moutier. March, 1909.
3. DOCTRINE DE L'APHASIE.—Bernheim (*Revue de medecine*, 1908, Nr. 9).
4. OBSERVATION D'APHEMIE PURE (anarthrie corticale).—Ladame et C. v. Monakow (*L'Encephale*, 1908, Nr. 3).
5. EIN FALL VON TRAUMATISCHER APHASIE MIT RECHTSSEITIGER HEMIPLEGIE BEI EINEM LINKSHAENDIGEN. TREPANATION HEILUNG.—Myiake (*Archiv. f. klin. Chir.*, LXXXVIII., 1909).
6. UEBER ZWEI FÄLLE VON TRAUMATISCHER MOTORISCHER APHASIE.—Kerchner (*Deut. Zeitschr. f. Chir.*, XCIV., H. 1, u. 2).
7. THE PRESENT ASPECT OF THE APHASIC QUESTION.—Liepmann (*Neurologisches Centralbl.*, 1909, Nr. 9).

The subject of aphasia has of late attracted the attention of neurologists and physiologists on account of the new conception of the problem advanced by Marie and his pupils. In the last three or four years the number of papers that have appeared upon the question is enormous. It is proposed in this review to state the conception of aphasia that has developed from the original work of Broca and Wernicke, and then to show in what way Marie has attempted to modify these conceptions, and further to quote from some of the recent literature on the subject to illustrate how these divergent views are regarded by some of the best qualified authorities on the subject. The best recent paper on the question is by S. A. K. Wilson, entitled "A Review of the Question of Aphasia." In this paper is contained a statement of the old view, a statement of Marie's view, and a critical discussion of the arguments, for and against the change. The paper is largely devoted likewise to a criticism of the most pretentious work in support of the Marie idea, that of Moutier. This work of Moutier's is by all odds the most comprehensive study that has appeared; it contains practically all the bibliography up to 1908, with a critical examination and discussion of all the pathological data which has been collected and studied in the laboratory of Marie.

By using these two sources, it will be possible to determine clearly the nature of the problem and likewise some idea of a newer view. It should be mentioned in the first place that there is no problem in the physiology of the nervous system that is any more complex than that of aphasia, and, moreover, that there is no problem which has been befogged to such an extent as has this one. The reason for the latter is that practically everyone who has attempted to write upon the question, has advanced his own theory in respect to the physiology of speech, and not only that, but many useless and misleading terms have been invented to explain this or that small variation from what had been known before. Diagrams have been devised to illustrate each author's view, and so



complex have they become that it is almost impossible to arrive at any just opinion as to the prevailing notion existing now.

The problem of aphasia in its simplest form and from the standpoint of teaching, might be stated in this way: In the left hemisphere in the majority of individuals there are four centres of speech. These centres are in no sense sharply limited areas, in the sense of a spinal cord reflex, for example, but are centres in a physiological and functional capacity; that is, certain areas, the size of which is unknown, have to do with the specialized function of speech. There are two receptive centres and two executive centres, as Wilson calls them. One of the receptive centres is where the auditory memory-images of words are stored, situated in the upper temporal convolution chiefly, towards its posterior end; the other is for the visual memory-images of words, and it occupies the neighborhood of the angular gyrus. Of the executive speech centres, one is the centre for motor speech, the so-called "Broca Centre," at the posterior end of the third left frontal convolution, while the other is a centre for the movements of writing, situated somewhat above the Broca Centre. Now, with these four centres almost all the different forms of aphasia can be explained about as satisfactorily as any physiological activity of the cortex can be explained. The recognition of these centres has been made by deductions from a large mass of clinical and pathological evidence. It is easily seen then that aphasia can be, in the first place, roughly divided into a lack of understanding for words and a lack of ability to utter words. In other words, there is a receptive difficulty and an executive difficulty. To the executive difficulty the name "motor aphasia" has been given, and to the other "sensory aphasia." The motor aphasic is practically dumb; he can articulate only a few words or syllables, though he understands what is said to him perfectly. The sensory aphasic can utter any number of words, phrases or sentences, but he fails to understand what is spoken to him, what he reads and what himself says. Now, Broca's notion, and for that reason it is called the "classical notion of aphasia," is that motor aphasia is caused by a lesion located in or near the Broca Centre, whereas sensory aphasia is located in or near the centre for the sensory speech mechanism. It is naturally supposed that in all speech disturbances as outlined above, that the peripheral mechanism of speech, lips, tongue, larynx, vocal cords, etc., are normal. Furthermore, one point must be considered, and that is that in all speech activity, whether of one kind or another, there must be mechanically an intellectual participation. Further, we must remember that in any disturbance of speech there must likewise be a disturbance of the intellectual sphere. In 1906 Marie commenced to publish a series of communications on the subject of aphasia. His views have been supported very largely by the remarkable theses of Moutier. Marie and his pupils uphold the following four propositions: 1, the third frontal convolution does not play any rôle in the disturbance of speech; 2, there is no essential distinction between motor and sensory aphasia; 3, pure or sub-cortical aphasia do not exist; 4, in every case of aphasia there is intellectual impairment. As opposed to Marie's view, Dejerine, in numerous publications, has supported the older view. The number of publications on the subject of aphasia since the first publication of Marie's work has grown tremendously. It is difficult to determine just where the prevailing opinion is.

Bernheim, of Nancy, states his idea somewhat as follows: There exists neither a centre for the memory of words nor for phonetic articulation, nor for the auditory and visual memory. Such portions of the cortex, as were formerly considered centres, are merely stations through which impulses go. The temporal and occipital cortex contains the

hearing and sight impressions; the frontal lobe merely interprets them. The frontal lobe creates internal speech, which is rendered audible by the bulbar nuclei.

Ladame and Monakow published an observation, clinically and anatomically studied, of a case of pure motor aphasia, or, according to Wernicke's nomenclature, cortical aphasia. The chief symptom was an absolute inability to talk, with the perseveration of understanding for words. The intelligence was at first not disturbed, but later declined. Anatomically they found lesions of all the fibre systems, association, commissural and projection systems. The Broca convolution and the operculum were both disturbed.

A number of recent surgical papers have been written in which the effect of traumatic lesions followed by motor aphasic symptoms were immediately relieved by the operation. Such as recorded by Myiake and Kerchner are to the point, although the lesions found roughly correspond to the cortical presentation as outlined in the classical theory that can not be accepted naturally as proof, for the reason that such brains can not be studied microscopically.

A paper in German corresponding somewhat to the English paper, so freely quoted in the beginning of this article, is by Liepmann, entitled "The Present Aspect of the Aphasic Question." In this paper the author objects to the term "classical theory" as presented by Broca, and suggests further that the descriptions in the text-book, as outlined by diagrams, should not be considered as seriously as they have been, owing to the necessity by devising schemes which will render clear the ideas of an author. He points out that in such descriptions the variations, anatomically or physiologically, can not be considered. In this paper is quoted an interesting observation in regard to the correctness of the original Wernicke theory; a case known as Hendschel, in which Wernicke had made a diagnosis of word deafness. Finally the case came to autopsy and the brain was cut in serial sections. It was found that in the cortex of the left temporal lobe there was a very widespread, old focus of disease, composed partly of a fibrous tissue and partly of a calcified substance. The word deafness was caused entirely by this temporal lobe lesion. Liepmann concludes his carefully considered paper somewhat as follows: The knowledge of aphasia must be developed further from the fundamental thoughts of Broca and Wernicke. The further development of the theory cannot be brought about except by the careful study of the pathological and physiological facts which relate to cerebral lesions. The notions of sub-cortical and trans-cortical must be changed. The facts which the study of apraxia and agnosia must be brought into relation with disturbances of speech. We are far away at present from a satisfactory view of the matter. A revision, such as Marie suggests, has especially the value of preventing investigators from feeling satisfied with what has already been obtained. It means a more careful study of the facts and the conclusions which have been deduced from them. It can be seen from these brief abstracts that the whole question of aphasia is still a matter of doubt, and it suggests further that in the future all cases which can throw any light upon the subject ought to be carefully investigated from the clinical point of view first of all, and such results recorded. After cases presenting aphasia come to autopsy, the study of the brain should be made carefully and in serial sections, studied microscopically and macroscopically, and the data thus obtained be added to the clinical disturbance noted. In this way, if the material has grown large enough, a theory for speech defects might be devised which shall be satisfactory from the clinical, as well as from the anatomical point of view.

## DIAGNOSTIC AND THERAPEUTIC NOTES.

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THE EARLY DIAGNOSIS OF PULMONARY TUBERCULOSIS.—Hausmann (*Deut. Arch. f. klin. Med.*, 1908, xciv., 595).—Hausmann was struck by the number of gastric cases in his practice which presented evidence of apical lesions. Those who raised no sputum he often sent to a lung specialist, and frequently his diagnosis of incipient pulmonary tuberculosis was reversed. Feeling that he was correct, he decided to examine the fasting stomach early in the morning for small particles of bronchial secretion. His first patient gave no history of cough, but had definite signs in one apex, the chief complaints being a feeling of fulness and weight in the epigastrium. A very small particle of mucous, enclosing pus cells and lymphocytes, and a few alveolar epithelial cells, were obtained from the stomach, and when stained showed about ten tubercle bacilli in a field. In the last two months he has had six similar cases, all with the same result. He urges the usefulness of this method and suggests that some of the so-called "closed" or "healed" cases may be shown by this means to be in reality "open" cases of tuberculosis.

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THE RELATION OF TETANY TO THE PARATHYROID AND TO CALCIUM METABOLISM.—W. G. MacCallum and C. Voegtlin (*Jour. Exp. Med.*, 1909, xi., 118).—W. G. MacCallum and C. Voegtlin report the experimental work on this subject, which they have been carrying on for some time, and conclude that there is an intimate relation between the various forms of tetany and the relative or absolute insufficiency of the parathyroid gland. These glands have a definite specific function. When extirpation of the parathyroids is complete, tetany appears, even in herbivora. Only a small amount of parathyroid tissue is required to prevent this. The effect of extirpation may be annulled by the re-injection of an extract of these glands, even from an animal of a widely different character. The parathyroid glands contain no considerable amount of iodine. In tetany there is apparently some disturbance of the composition of the circulating fluids, ordinarily prevented by the normal secretion of the parathyroid; this change in the composition disarranges the balance of the mineral constituents of the tissues. Calcium salts bear an important relation to the excitability of the central nervous system; their withdrawal leaves the nerve cells in a state of hyperexcitability, which can be made to disappear by supplying them with a solution of a calcium salt. Tetany may be regarded as an expression of hyperexcitability of the nerve cells from some such cause; the injection of a solution of a salt of calcium into the circulation of an animal in tetany promptly checks all symptoms and restores the animal to an apparently normal condition. Sodium and potassium, as well as some other salts that have been studied, have no such beneficial effects. This effect of calcium is of value in human therapeutics as a means of tiding over the period of acute parathyroid insufficiency until remnants of parathyroid tissue can recover their function or new parathyroid tissue be transplanted. The metabolism in parathyroidectomized animals shows, among other things,



a marked reduction in the calcium content of the tissues, especially of the blood and brain during tetany, and an increased output of calcium in the urine and feces on the development of tetany. In general, then, the role of the calcium salts in connection with tetany may be conceived of as follows: These salts have a moderating influence upon the nerve cells; the parathyroid secretion in some way controls the calcium exchanges in the body. It may possibly be that in the absence of the parathyroid secretion substances arise which can combine with calcium, abstract it from the tissues, and cause its excretion, and that the parathyroid secretion prevents the appearance of such bodies. The mechanism of the parathyroid action is not determined, but the result, the impoverishment of the tissues with respect to calcium and the consequent development of hyperexcitability of the nerve cells and tetany, is proved. Only the restoration of calcium to the tissues can prevent this.

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THE ANTITRYPTIC POWER OF THE SERUM OF DIAGNOSTIC VALUE.—L. Ambard (*Semaine med.*, 1908, xxviii., 532).—Ambard notes that it has been known for some time that serum possesses the property of inhibiting the action of trypsin, thus checking the digestive power of the pancreatic juice. At one time this power was supposed to be of definite value in the diagnosis of cases of cancer, but later it has been found in other conditions. In animals this antitryptic power is increased by the injection of trypsin, and in man by the ingestion of pancreatin. This increased power and its compensatory hypersecretion forms a common link between cancer, nephritis, etc., and other conditions in which the antitryptic power of the serum is increased.

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THE DIFFERENTIAL DIAGNOSIS OF ACUTE LEUKEMIAS.—Schultze (*Munch. med. Woch.*, 1909, lvi., 167).—Schultze refers to the difficulty that exists in differentiating acute lymphoid and myeloid leukemias. In the chronic cases the differential diagnosis is easy, but acute cases are quite the reverse, for here one has to differentiate the lymphoblast, the antecedent of the lymphocytes, from the myeloblast, which is the parent cell of the granular leukocytes. The two cells are practically identical in size, each possesses strongly basophilic protoplasm, and their nuclei are similar. Schridde describes a narrow, clear border about the nucleus of the lymphoblast, but for most observers this is not a practical differential point. Schultze makes use of the so-called oxydase reaction (indophenoblausynthese) for differentiating these cells and especially for differentiating the tissues of lymphoid from those of myeloid leukemia. For carrying out this reaction a 1 per cent. aqueous solution of ax-naphthol and 1 per cent. aqueous solution of dimethylparaphenylendiamin (E. Merck) are needed. The naphthol, which is only slightly soluble, is warmed and then filtered after cooling. The two solutions, when united, form a blue color under the action of light through an oxidative synthesis. In the presence of an oxidative ferment the process is greatly accelerated. Myeloid tissue, when treated with these solutions, is stained blue, owing to the presence of an oxidative ferment, while lymphoid tissue remains unstained. The method is especially applicable to tissue.

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THE VALUE OF THE LEUKOCYTE AND DIFFERENTIAL COUNTS IN APPENDICITIS.—Pease (*Annals of Surgery*, 1909, xlix., 385).—Pease reports a study of 300 appendicitis cases treated in the Presbyterian Hospital of

New York, in which leukocyte and differential counts had been made previous to, and the pathological lesions demonstrated at, operation. The more severe the pathological lesion the higher the leukocyte count and the higher the percentage of polymorphonuclear cells in the differential count. A good many of the cases of general peritonitis, however, gave a low leukocyte count, and thus reduced the general average below that of the average for abscess cases. The explanation is probably to be found in the overwhelming dose of infection which reduced the body reaction. It is impossible, therefore, to decide the pathological lesion present from the leukocyte count alone. The pathological lesion can be judged more accurately from the differential count than from the leukocyte count. A polymorphonuclear count of between 85 and 90 per cent. indicates the presence of a severe process; above 90 per cent., a dangerous condition probably complicated by peritonitis; below 80 per cent., safety for the time being; between 80 and 85 per cent., a doubt. These rules held good for about four-fifths of the cases, there being many exceptions to each rule. One point, and probably the most important that this investigation has taught Pease, is that it is impossible to decide from the blood count alone what pathological lesion we shall find, or even to determine whether the case is severe or not. There are many exceptions and these we must learn to interpret by other means at our command.

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THE LOEWI REACTION.—A. Bittorf (*Zentralblatt f. innere Medizin*, 1909, No. 2).—According to Loewi, instillation of a solution of adrenalin into the eyes of diabetics frequently causes dilatation of the pupils. Loewi explains this phenomenon by the absence of stimuli emanating from the pancreas. The author's investigations do not confirm Loewi's opinion.

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SIGNS OF EARLY DISEASE OF THE THYROID GLAND.—G. R. Murray (*British Med. Jour.*, February 13, 1909).—The mild types of myxedema, which result from a partial fibrosis of the thyroid gland, occur not uncommonly in women between the fortieth and fiftieth years of age. When the fibrosis is complete, a fully developed case of myxedema results. Some cases of fibrosis never become complete and are never recognized, and so there results a prolonged period of illness for the unfortunate sufferers. The symptoms which result from partial thyroid fibrosis usually come on very gradually between the fortieth and fiftieth years of age, and are frequently mistaken for those symptoms occurring during the menopause. There is a gradual loss of physical and mental energy, so that every action appears to require a special effort. Memory is defective, visual hallucinations are common, and sensations of cold are complained of, although the body temperature is normal. Inspection shows that the facial appearance is somewhat altered. The cheeks are rounded and full, and a central pink flush appears, imparting a fictitious appearance of improved health and nutrition. There is also a small amount of swelling of the eyelids, which has a translucent appearance like the swelling due to a subcutaneous edema, but does not pit on pressure. The face may be pale and waxy, or it may have a pale-yellow tinge. The lips are sometimes swollen. The subcutaneous swelling on the body is generally not sufficient to attract the patient's attention, though it may be found on careful examination. In the supraclavicular regions, the swelling is sometimes distinct. The back of the hands are

rounded and the feet may show a similar condition. The thyroid may be found to be slightly diminished in size.

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THYMUS ENLARGEMENT ASSOCIATED WITH NERVOUS SYMPTOMS.—O. Lerch (*Med. Rec.*, March 6, 1909).—Author proposes the term "thymoke-sis" (abnormally large thymus) for thymus enlargement. In adults, the gland may be well outlined by percussion up to extreme old age. The size of the thymus when full of blood and lymph varies from 2 to 3 cm. to 12 and 14 cm. at the base. The apex parallel to the base may adjoin cardiac dulness. The area of dulness is represented by a trapezoid, the lateral sides of which are slightly curved, and which is sometimes not larger than a half dollar. In order to percuss the gland it is best to place the patient in the erect position with the head thrown back. Light as well as deep percussion may be employed; both methods will give the contours of the gland, but the results of the two methods do not always correspond in every detail. For controlling purposes palpating percussion may be resorted to.

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PERCUSSION OF ABSOLUTE CARDIAC DULNESS AND ITS VALUE IN THE DETERMINATION OF THE SIZE OF THE HEART.—Schiffer and Weber (*Deutsches Archiv. f. klin. Med.*, Vol. LXXXIV., No. 5).—Orthodiagraphic control of absolute cardiac dulness evinces that it is not always possible to depend on the latter when estimating the size of the heart in normal cases and such of cardiac disease. In addition to the absolute cardiac dulness, in the interpretation of which great care must be exercised, the determination of the relative dulness is essential when an exact cardiac examination is made.

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PARALYSIS OF THE LEFT RECURRENT LARYNGEAL NERVE IN MITRAL VALVE DISEASE.—W. Osler (*Montreal Med. Jour.*, February, 1909).—In mitral valve disease with great dilatation of the left auricle and compression of the left recurrent laryngeal nerve, there may be a combination of symptoms and physical signs most suggestive of aneurysm. In these cases, autopsy shows that the nerve is compressed between the greatly dilated auricle and the aorta. Paroxysms of orthopnea, cyanosis, the cracked voice, the paralyzed vocal cord, and on inspection a pulsation in the second left interspace, may make a most deceptive combination.

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MOVABLE CECUM AND CHRONIC APPENDICITIS.—Wiemann (*Deutsche med. Wochen.*, 1909, No. 4).—The cause of the pains in chronic appendicitis may be situated in the cecum. There may be accumulation of feces in the same as the consequence of spastic constipation; this fecal accumulation may give rise to pressure upon the nerves. The pains may also be due to a neuritis on a toxic basis, called forth by enterotoxins. Author concurs with a former writer that a long, too freely movable cecum may cause the pain. For this reason the pain may continue after appendectomy has been performed. In such cases it does not suffice to remove the appendix but the cecum must also be securely anchored, so that the patient is rendered pain-free.

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DIAGNOSTIC AND THERAPEUTIC VALUE OF LUMBAR PUNCTURE.—E. Bramwell (*Clin. Studies*, January, 1909).—Lumbar puncture is of value



in the diagnosis of meningitis from hysteria, meningism, brain abscess, etc., and for differentiating the various forms of meningitis, especially the obscure cases of cerebro-spinal fever in which isolation and serum treatment are indicated. Repeated punctures have a beneficial effect in sero-purulent or purulent meningeal inflammation. Detection of blood in the cerebro-spinal fluid may determine a doubtful case of meningeal hemorrhage. Diagnosis of neurasthenia from general paralysis of the insane is often important as is frequently *tabes dorsalis*, or of cerebro-spinal syphilis from other uncertain affections. A pronounced cerebro-spinal lymphocytosis is the rule in these syphilitic and parasymphilitic affections of the nervous system, its detection being an indication for vigorous antisymphilitic treatment. Trypanosomes have constantly been found in the cerebro-spinal fluid of sleeping sickness, while tumor cells from malignant growths of the meninges are occasionally encountered. Tuberculous meningitis in which the tubercle bacillus had been identified in the cerebro-spinal fluid, has resulted in recovery.

## CORRESPONDENCE.

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### PARIS LETTER.

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By AUGUSTE A. HOUSQUAINS, M. D.

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#### ACCIDENTAL INJURIES TO WORKMEN.

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The law which has been in force in France since 1898, to indemnify workmen who are the victims of accidents occurring during work, has had the unforeseen consequences of contributing, in a great measure, to a revision of the work of Charcot. The rigorous advocacy of this law has resulted in a large number of researches in hysteria. We know the important works of Babinski on this question. Though post-traumatic nervous disturbances have existed at all times, their frequency has increased prodigiously in France, as well as in all countries where there exists insurance against industrial accidents. There is a tendency to include under the comprehensive term "traumatic neurosis," the disturbances which are in reality either traumatic hysteria, traumatic neurasthenia, or even traumatic hystero-neurasthenia, but which should be differentiated from "traumatic neurosis" properly called. Therefore, there is considerable danger in making the correct diagnosis, since prognosis is subordinated to diagnosis. The result of all this is that, according to Professor Brissaud, the decisions from the point of view of jurisprudence are from the start inconsequential and contradictory.

According to the present conception of hysteria, the only symptoms attributable to it, outside "an attack of nerves," are limited to certain phenomena, which can be provoked by suggestion or suppressed by persuasion. This has been called by Babinski "pithiatism." The contraction, the palsy, and the anesthesia, the nature of which is no longer disputed as being pithiatic, constitute the fundamental stigmata of traumatic hysteria. But how are we going to differentiate these symptoms from a malingered hysteria?

In neurasthenia it is necessary to distinguish between two groups of disorders: First, those which are frankly nervous, and of a well-authenticated functional order—for example, denutrition, gastro-intestinal symptoms, visceral ptosis, cardio-vascular symptoms, tachycardia, and a lowering of the arterial tension; second, those which are beyond control, that is to say, are essentially pithiatic, and can be relieved only by suggestion—amyosthenia, cephalalgia, topoalgia, vertigo and insomnia. Now the appreciable visceral disorders are absolutely exceptional in pretended traumatic neurasthenia. "True neurasthenia," says M. Bernheim, "is independent of volition; it occurs in subjects who are not conceited or self-centered, but who have force of character and moral energy." The malady is auto-toxic or toxi-infectuous. It is often constitutional, and

is linked to a dyscrasia which can be brought into action by such moral causes as emotional or traumatic shock. One might define the condition as psychoneurosis, but one cannot influence a brain to rid the organism of the toxins, developed by the microbes or created by a nutritive dyscrasia. On account of these traits we see that a false neurasthenia, of a pithiatic nature, ought to be classed with hysteria, of which it is an integral part.

Associated with each other as they really are, hysteria and traumatic neurasthenia remain pithiatic; they manifest themselves under varied aspects, thanks to the special education of the wounded and the fixed idea which possesses them. Thus is explained the contagiousness of these disorders; and we know that traumatic neurasthenia has local epidemics similar to hysteria. All the accidents which supervene in consequence, or at the time of an injury, become so many pretexts for the most diverse suggestions. "By an insensible gradation," says M. Brissaud, "neurasthenic cephalalgia can become hysterical; localized amyosthenia develop into paralysis, and numbness pass into anesthesia, under the influence of conversation or even of a monologue to which the wounded is so complacently attentive." Again, neurasthenia is one of those maladies so thoroughly exploited that nearly every imaginative person is capable of maleficent deeds. The result of this medical education of the wounded is the following: A man may have a slight inclination to take up his work again, but this is outbalanced by a strong inclination to remain sick. He firmly believes that the hystero-traumatism, with which his doctor has declared him affected, is still too grave to permit him to resume his work or even to attempt a trial. A fixed idea gives birth to the conviction that he ought to recuperate all his strength without any effort on his part. If he has had a fracture of the leg, for example, he looks forward with joy to the day when the apparatus shall be removed, and it will be possible for him to walk; nevertheless, when the time arrives, he will be unwilling to get out of bed to make an attempt to walk. He insists that he ought to be in the same condition as before the accident. The pain and the inconveniences following a fracture are to him real complications. The inevitable tingling when the limb is again used are precursory symptoms of paralysis; anxiety, phobias and sensory-motor disorders complete the syndrome of a hystero-neurasthenia. But all this is only the consequence of a fixed idea, a mental creation which, suggested or spontaneously acquired, makes for "pithiatism." The fixed idea in consequence of a sort of inhibition of the volition is pushed to the condition that may be called "unconscious simulation," despite the fact that there is considerable oddity in coupling these words together. To explain the suggestibility which causes traumatic hystero-neurasthenia, a moral shock is necessary. Is it then necessary to enact a law to indemnify a moral shock? If so, the principle of indemnifying a wounded workman would apply equally as well to the accident of which he is the spectator, as to the accident of which he is the victim. At present, according to the letter of the French law, it is certain that he cannot receive indemnity unless he has received a personal wound.

It remains to add a few words to what Oppenheim has designated as "traumatic neurosis" properly called. This appellation is of a later date than the one used by English and American physicians and surgeons when describing nervous disorders, supervening after severe traumatic commotions. Walton in 1883, Putnam in 1884, Page in 1885, signalized a neuropathic and psychic syndrome, especially frequent after railway collisions, and which merits as much the name of "railway-brain" as "rail-



way-spine," since at times it unites the signs of these two clinical types. We cannot deny the hypothesis that hysteria and neurasthenia may combine to make this new entity. Undoubtedly, moral shock complicates a wound.

In reality, even when the stigmata of hysteria exist, traumatic neurosis continues with or without hysteria and neurasthenia. In traumatic neurosis, the secondary nervous disorders follow the incipient disorders too quickly to allow of a period of meditation that might give rise to a fixed idea. After the period of loss of conscience, which happens, for example, from a fall on the head, the subject comes to himself only slowly and incompletely; he is stunned, confused, without memory and without firm intent; little by little the physical state is ameliorated, but the psychic state remains far from normal; the genuine efforts he makes to recuperate his activity weaken and irritate him, and depression is accentuated. The wounded becomes listless; amnesia, perplexity, a sort of torpor engulf him; a permanent hebetude, indicative of a heavy head and aching brain, produces the most complete passivity. If we persevere in the way of re-education and also if the wounded workman is isolated, this state of a veritable psychosis is susceptible of modification in a favorable fashion; at least, the wounded does not fall into a state of systematic mutism.

Certain somatic characteristics are excellent guides in making the diagnosis; permanent tachycardia, atonic dyspepsia, fetidness of the breath with a coating of the tongue, phosphoturia, alternating reddening and pallor of the face.

What is the cause of traumatic neurosis? M. Brissaud thinks that though a violent shake-up from which an organism may suffer, may not cause fatal alterations in the centers—hemorrhage and softening—it is nevertheless capable of causing histological modifications in the centers: a sudden wrenching, more or less lasting, of the articulations of the neurones.

These, then, are the nervous disorders to be observed after traumatism. The ten years in which the law, indemnifying accidental injuries to workmen, has been in force, have profoundly modified our knowledge of hysteria, neurasthenia, and traumatic neurosis; and it is not going beyond the truth to state that the multiplicity of facts, daily observed in connection with the exercise of this law, is casting greater and greater light on the obscurities of these diseases.

June 10.

## OBITER DICTA FROM FOREIGN JOURNALS.

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### THE MARRIAGE OF SUBJECTS WITH CARDIAC DISEASE.

"The subject of marriage by those who are afflicted with cardiac disease," says Dr. Louis Rénou, in the *Gazette des Hopitaux* of March 6th, "is an important one to all doctors because quite often they are consulted by parents as to whether or no their offspring should marry. In the interests of the sick and of society, it is incumbent upon us to ascertain the best means of settling this vital question, for the matter is of enough import to engage the attention of all modern sociologists. Many have expressed the idea that all marriages should be preceded by a medical examination, and M. Cazalis, in his book, 'Science and Marriage,' says 'the day is not far distant when families, having decided upon a marriage, will refer the matter to their respective doctors, just as to-day they consult their notaries in regard to money matters; and the doctors' opinion will take precedence of the notaries' opinion, since the question of health is more important than the question of money.' Furthermore, the idea is beginning to take root with the thinking part of the world that we ought to exercise as much care in the choice of human progenitors as is done in the domestic zoötechny. In my book on 'Popular Maladies,' I have already insisted on these points, and I think they ought to be recalled now, since they constitute a necessary preamble to this very interesting subject."

The question of the marriage of subjects with heart disease should be separately envisaged in the man and in the woman. The personal risks in the man are almost nil; in fact, the calm of a conjugal existence is of benefit, since its tranquility is greater than was the case when he led the life of a bachelor. Granting this, would his offspring be in no danger of inheriting the paternal cardiac defect? Beyond a doubt a father affected with cardio-sclerosis, interstitial nephritis and hereditary aortic disease can, as has been shown by Huchard, engender children affected with similar lesions. This morbid heredity takes the shape of a hereditary renal debility, a matter recently insisted upon by Castaigne.

The question of morbid heredity, as a resultant of valvular cardiac disease, has been widely discussed. Although Huchard has made light of the matter, Hirtz and his pupil Servin report a number of cases in children in which a congenital mitral stenosis could be attributed to transmission. And since this is a congenital malformation there may be other malformations and stigmata of degeneration present. There are two important factors which should influence the doctor in his decision—the age of the patient and the nature of the disease. An aortic aneurysm and a valvular insufficiency that is poorly compensated, are contraindications against marriage; the evidence against the step is so overwhelming that doctors are not often asked to express an opinion. A congenital mitral stenosis, especially in a person advanced in years, should be a warning against marriage, since a patient aside from personal risks which he might encounter, may become the founder of a hereditary dystrophy. Before allowing the marriage of a subject who has arterio-sclerosis with great hypertension, a doctor should submit him



to a regimen that would exclude the habitual factors producing hypertension, such as tobacco, coffee and animal food. Finally, a man affected with cardiac disease should not espouse a woman affected in a like manner, nor should he marry a person closely related to him, since consanguinity favors the transmission of hereditary dystrophy. With the exception of these restrictions, the marriage of male cardiacs need not be interdicted.

The important part of this intricate question is the marriage of women affected with cardiac disease. Besides the risks which menace the female cardiac during pregnancy, confinement and lactation, there is danger of the child succumbing during pregnancy or confinement; or surviving with a hereditary cardiac defect. Until recently, the attitude of the doctor was simplicity itself. All the afflicted were excluded from marriage. Peter's aphorism had become a sort of law. He had formulated it *apropos* of two cases in which pregnancy was interrupted by grave accidents. These he designated by the name of gravidocardiac accidents. Dr. Rénon, when he was head of Professor Bar's laboratory in 1894, saw a woman affected with a mitral lesion succumb to acute edema of the lungs; the urine and the serum of the blood showing considerable toxicity. But these accidents are not always fatal during the pregnancies of cardiacs. Pouliot, in his doctor's thesis (1904), "Accidents which complicate Diseases of the Heart during Pregnancy," illustrates the general acceptance of Peter's Law. Leyden is cited, since, according to him, the mortality of pregnant women affected with heart disease is 55 per 100; McDonald, on account of the higher percentage of 60.7 per 100; and Berry Hart, because he sets the number still higher, 87.5 per 100. Therefore, following the author's argument, the marriage of female cardiacs should be discouraged.

In contradistinction to the foregoing, the fact obtains that cardiacs are known to become multiparæ without any untoward results, and when a bad result does happen, it is only after many confinements. Pouliot cites the case of a cardiac who died suddenly in her eighth pregnancy. Budin mentions a case succumbing in her fifteenth pregnancy and Porak states that the number of deaths during actual labor is inconsiderable. The following important statistics are worthy of consideration: Out of 5,000 confinements Vinay found 80 affected with heart disease; of these 80, 72 had no accident, 6 suffered with asystole, 2 with pulmonary edema; of the 8 affected with cardio-pulmonary disturbances, 3 only succumbed. Demelin observed 64 with cardiac disease in a total of 5,162 confinements. Fellner in Schauta's clinic in Vienna auscultated systematically 900 women; 22 had cardiac lesions; only 2 were troubled during their pregnancy. Champetier de Ribes, out of 5,998 confinements, encountered 68 with cardiac disease; 2 succumbed. Dr. Rénon has observed 3 afflicted with mitral stenosis, a lesion that does not seem to produce untoward results during pregnancy. In sum, one can say that the majority of pregnant women tolerate their cardiopathy, a small number only presenting grave disturbances.

What accidents occur? There are two groups: cardiac insufficiency and acute edema of the lungs. Cardiac insufficiency manifests itself ordinarily towards the end of the first half of pregnancy. Its incipient sign is dyspnea, coming on at first after some exercise, later manifesting itself when the body is in repose, and finally becoming continuous. It is accompanied by profound agony and palpitations more or less violent. The cardiac swoon develops very quickly, the asystole at first is transitory and then transforms itself into amyocardia. Often as a final occurrence there is repeated hemoptysis, especially frequent in the second



half of pregnancy. The acute edema occurs ordinarily later than the cardiac insufficiency, is present in the last months of pregnancy and during labor, and sometimes even after delivery. It manifests itself brusquely by dyspnea, agony, a hacking, recurring cough with a bloody froth, and always terminates in death in a few hours. Pouliot insists justly on a special syndrome which he calls by the name of gravidocardiac cachexia. It occurs with those multiparæ who have borne many children and who suddenly, during the three weeks following confinement, present signs of cardiac insufficiency approaching rapidly to a state of irreducible asystole of extreme gravity. But the number of cardiac women who perfectly support the fatigues of pregnancy and confinement is so large that Peter's Law needs revision (Pouliot: The Revision of Peter's Law. *Arch. gén. de méd.*, fév. 1908). Potain, Vaquez and Vinay no longer forbid the marriage of cardiacs. Huchard determines this question by the exercise of good judgment. Pouliot proscribes it only in women in whom the decided disturbances carry a certainty that they cannot stand the trials of pregnancy. He interdicts it also in young women afflicted with cardiac atresia, and in those who have a slight cardiac insufficiency, only evidenced by a passing edema, oppression upon effort and a temporary albuminuria; in those who have a decidedly narrow stenosis or an aortitis with double lesion of the orifice; and finally, when cardiopathy, though well compensated, coexists with the theoretic malformations, nephritis or pulmonary tuberculosis.

The arterial tension is also an important element in the determination of the state of the sick. A decided hypotension (8 to 9 centimeters, according to the apparatus of Vaquez or Amblard), a notable hypertension of 16 to 17 centimeters measured by the same means, are good counterindications against marriage. But these disturbances in the tension, to carry weight as an argument against marriage, must not be ephemeral, but continuous; and a doctor should not be stubborn of his opinion until the repeated examinations establish the permanence of a vascular trouble. When cardiopathy is neither hereditary nor characteristic of the family; when there are no other malformations in the family; when the prospective husband is not afflicted with heart disease, nor closely related to his future wife, marriage is permissible.

The social position of the woman is a matter of much importance. Those who are exposed to fatigues, to prolonged and violent efforts, cannot undergo with impunity the coexistence of pregnancy and a cardiac lesion. In case a cardiac is advised to marry, and pregnancy occurs, precautions should be taken. It is important to supervise the alimentation, examine frequently the urine to ascertain whether albumin is present or chlorides retained, since Bar has shown that eclampsia often occurs in these conditions. All fatiguing efforts should be shunned and from the fourth month on complete repose should be enforced. During labor the surveillance of the patient should be redoubled; if there is dyspnea, it is necessary to hurry on labor by version or the application of forceps. At the moment of delivery, it is wrong to hasten matters, and to arrest too quickly the bloody oozing which always accompanies labor. This bloody discharge is more favorable than might at first be thought. Finally lactation should be discouraged, because it might be the origin of great fatigue, depression and weariness. The therapeutics in these cases consists of cardiac tonics: digitalin, spartein, oil of camphor. Pulmonary edema should be treated by general blood-letting. In the presence of grave accidents, if pregnancy is not terminated, it is necessary to resort to abortion or a premature and rapid delivery.



## BOOK REVIEWS.

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**DISEASES OF THE SKIN AND THE ERUPTIVE FEVERS.** By Jay Frank Schamberg, M. D., Professor of Dermatology and Infectious Eruptive Diseases in the Philadelphia Polyclinic and College for Graduates in Medicine. Octavo of 534 pages, illustrated. Philadelphia and London: W. B. Saunders Company. 1908. Cloth, \$3.00 net.

Dr. Schamberg's book will favorably compare with the best books on skin diseases. It contains more thorough descriptions of the eruptive fevers from a dermatological standpoint and of the exanthemata met with in typhoid fever, typhus fever, epidemic cerebrospinal meningitis, influenza, malaria, rheumatic fever, dengue, miliary fever, angina, etc., than do the other English treatises. This feature makes the book especially valuable for dermatologists, who are often called in consultation for just these eruptions. We congratulate the author on his work and predict several editions.

**ADENOMYOMA OF THE UTERUS.** By Thomas Stephen Cullen, Associate Professor of Gynecology in the Johns Hopkins University, etc. Illustrated by Becker and Horn. Philadelphia and London: W. B. Saunders Company. Price, cloth, \$5.00 net.

After the publication of von Recklinghausen's work on adenomyoma in 1896, considerable controversy arose as to the origin of this peculiar variety of uterine myoma. To-day, opinion seems definitely settled against Recklinghausen's theory, and this, to a very large extent, is due to the very painstaking investigations of Cullen. This book gives an exact account of all the cases of uterine adenomyoma observed by the author. In almost every case of diffuse adenomyoma he was able to trace directly the origin of the glands in the tumor to the endometrium, thus proving that these diffuse myomata are the result of a flowing outward of the normal uterine mucosa.

The book is beautifully illustrated, is printed in a type larger than that customarily used, and is a splendid specimen of the most advanced standard of make-up of American text-books.

**MEDICAL GYNECOLOGY.** By Samuel Willis Bandler, M. D., Adjunct Professor of Diseases of Women, New York Post-Graduate Medical School and Hospital, etc., etc. Philadelphia and London: W. B. Saunders Company. Price, cloth, \$5.00 net.

In no field of medicine is conservative treatment of greater value; but, combined with this, there is needed a knowledge of the relation of normal and pathologic genital functions to the general physical and psychic health of woman. The organism and mode of life of woman are such that, aside from the diseases and injuries to which she is liable, physiologic processes, heredity, predisposition, mental perturbation, the emotions, marital relations, etc., have an important bearing, and, therefore, the physician who enters into a study of these factors becomes a far better judge of the meaning of symptoms.

In this volume operative procedures have been viewed as a last resort in those numerous conditions, where medical means can accomplish so much.

Who could deny the correctness and convincing truth of these statements made by the author in the preface of his interesting work?

This book will be eagerly studied by every man professing to be a gynecologist, and, it is hoped, will be considered a most forceful argument against the ridiculous claim of some American surgeons that gynecology is only a special branch of general surgery.

**HEALTH AND BEAUTY.** By John Shoemaker, LL.D., Clinical Professor of Diseases of the Skin in the Medico-Chirurgical College of Philadelphia, etc. Octavo of 476 pages. Philadelphia: F. A. Davis Company, Publishers. 1908.

This book is of interest to the "beauty doctor," women of leisure and laymen. While the chapters on beauty from an historical, artistic and hygienic standpoint, and the chapters on hygiene, education of the body, cultivation of the mind, training, ventilation, dress, the bath, etc., are not only interesting but have some value for the student, the chapters on diseases of the skin and the eruptive fevers are of little worth, and therefore it is to be deplored that Dr. Shoemaker used his valuable time in writing a popular "cutaneous therapeutics," since it will do no harm to the lay-reader in giving him a wrong impression of the subject. It is difficult enough for physicians to make the right diagnosis in a great many diseases of the skin; hence, to give advice to the laity as to diagnosis and treatment is a mistake in judgment.